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THE PSYCHOLOGICAL REVIEW

A MATHEMATICAL MODEL FOR SIMPLE LEARNING

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Harvard University [2]

Introduction

Mathematical models for empirical phenomena aid the development of a science when a sufficient body of quantitative information has been accumulated. This accumulation can be used to point the direction in which models should be constructed and to test the adequacy of such models in their interim states. Models, in turn, frequently are useful in organizing and interpreting experimental data and in suggesting new directions for experimental research. Among the branches of psychology, few are as rich as learning in quantity and variety of available data necessary for model building. Evidence of this fact is provided by the numerous attempts to construct quantitative models for learning phenomena. The most recent contribution is that of Estes (2).

In this paper we shall present the basic structure of a new mathematical model designed to describe some simple learning situations. We shall focus attention on acquisition and extinction in experimental arrangements using straight runways and Skinner boxes, though we believe the model is more general; we plan to extend the model in order to describe multiple-choice problems and experiments in generalization and discrimination in later papers. Wherever possible we shall discuss the correspondence between our model and the one being developed by Estes (2), since striking parallels do exist even though many of the basic premises differ. Our model is discussed and developed primarily in terms of reinforcement concepts while Estes' model stems from an attempt to formalize association theory. Both models, however, may be re-interpreted in terms of other sets of concepts. This state of affairs is a common feature of most mathematical models. An example is the particle and wave interpretations of modern atomic theory.

We are concerned with the type of learning which has been called "instrumental conditioning" (5), "operant behavior" or "type R conditioning" (10), and not with "classical conditioning" (5), "Pavlovian conditioning" or "type S conditioning" (10). We shall follow Sears (9) in dividing up the chain of events as follows: (1) perception of a stimulus, (2) performance of a response or instrumental act, (3) occurrence of an environmental event,

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and (4) execution of a goal response. Examples of instrumental responses are the traversing of a runway, pressing of a lever, etc. By environmental events we mean the presentation of a "reinforcing stimulus" (10) such as food or water, but we wish to include in this category electric shocks and other forms of punishment, removal of the animal from the apparatus, the sounding of a buzzer, etc. Hence any change in the stimulus situation which follows an instrumental response is called an environmental event. A goal response, such as eating food or drinking water, is not necessarily involved in the chain. It is implied, however, that the organism has a motivation or drive which corresponds to some goal response. Operationally speaking, we infer a state of motivation from observing a goal response.

Probabilities and How They Change

As a measure of behavior, we have chosen the probability, p, that the instrumental response will occur during a specified time, h. This probability will change during conditioning and extinction and will be related to experimental variables such as latent time, rate, and frequency of choices. The choice of the time interval, h, will be discussed later. We conceive that the probability, p, is increased or decreased a small amount after each occurrence of the response and that the determinants of the amount of change in p are the environmental events and the work or effort expended in making the response. In addition, of course, the magnitude of the change depends upon the properties of the organism and upon the value of the probability before the response occurred. For example, if the probability was already unity, it could not be increased further.

Our task, then, is to describe the change in probability which occurs after each performance of the response being studied. We wish to express this change in terms of the probability immediately prior to the occurrence of the response and so we explicitly assume that the change is independent of the still earlier values of the probability. For convenience in describing the step-wise change in probability, we introduce the concept of a mathematical operator. The notion is elementary and in no way mysterious: an operator Q when applied to an operand p yields a new quantity Qp (read Q operating on p). Ordinary mathematical operations of addition, multiplication, differentiation, etc., may be defined in terms of operators. For the present purpose, we are interested in a class of operators Q which when applied to our probability p will give a new value of probability Qp. As mentioned above, we are assuming that this new probability, Qp, can be expressed in terms of the old value, p. Supposing Qp to be a well-behaved function, we can expand it as a power series in p:

$$Qp = a_0 + a_1 p + a_2 p^2 + \cdots \quad (1)$$

where $a_0, a_1, a_2, \cdots$ are constants independent of p. In order to simplify the mathematical analysis which follows, we shall retain only the first two terms in this expansion. Thus, we are assuming that we can employ operators which represent a linear transformation on p. If the change is small, one would expect that this assumption would provide an adequate first approximation. Our operator Q is then completely defined as soon as we specify the constants a_0 and a_1; this is the major problem at hand. For reasons that will soon be apparent, we choose to let $a_0 = a$ and $a_1 = 1 - a - b$. This choice of parameters permits us

to write our operator in the form

$$Qp = p + a(1 - p) - bp. \quad (2)$$

This is our basic operator and equation (2) will be used as the cornerstone for our theoretical development. To maintain the probability between 0 and 1, the parameters a and b must also lie between 0 and 1. Since a is positive, we see that the term, $a(1 - p)$, of equation (2) corresponds to an increment in p which is proportional to the maximum possible increment, $(1 - p)$. Moreover, since b is positive, the term, $-bp$, corresponds to a decrement in p which is proportional to the maximum possible decrement, $-p$. Therefore, we can associate with the parameter a those factors which always increase the probability and with the parameter b those factors which always decrease the probability. It is for these reasons that we rewrote our operator in the form given in equation (2).

We associate the event of presenting a reward or other reinforcing stimulus with the parameter a, and we assume that $a = 0$ when no reward is given as in experimental extinction. With the parameter b, we associate events such as punishment and the work required in making the response. (See the review by Solomon [11] of the influence of work on behavior.) In many respects, our term, $a(1 - p)$, corresponds to an increment in "excitatory potential" in Hull's theory (6) and our term, $-bp$, corresponds to an increment in Hull's "inhibitory potential."

In this paper, we make no further attempt to relate our parameters, a and b, to experimental variables such as amount of reward, amount of work, strength of motivation, etc. In comparing our theoretical results with experimental data, we will choose values of a and b which give the best fit. In other words, our model at the present time is concerned only with the form of conditioning and extinction curves, not with the precise values of parameters for particular conditions and particular organisms.

Continuous Reinforcement and Extinction

Up to this point, we have discussed only the effect of the occurrence of a response upon the probability of that response. Since probability must be conserved, *i.e.*, since in a time interval h an organism will make some response or no response, we must investigate the effect of the occurrence of one response upon the probability of another response. In a later paper, we shall discuss this problem in detail, but for the present purpose we must include the following assumption. We conceive that there are two general kinds of responses, overt and non-overt. The overt responses are subdivided into classes A, B, C, etc. If an overt response A occurs and is neither rewarded nor punished, then the probability of any mutually exclusive overt response B is not changed. Nevertheless, the probability of that response A is changed after an occurrence on which it is neither rewarded nor punished. Since the total probability of all responses must be unity, it follows that the probability gained or lost by response A must be compensated by a corresponding loss or gain in probability of the non-overt responses. This assumption is important in the analysis of experiments which use a runway or Skinner box, for example. In such experiments a single class of responses is singled out for study, but other overt responses can and do occur. We defer until a later paper the discussion of experiments in which two or more responses are reinforced differentially.

With the aid of our mathematical

operator of equation (2) we may now describe the progressive change in the probability of a response in an experiment such as the Graham-Gagné runway (3) or Skinner box (16) in which the same environmental events follow each occurrence of the response. We need only apply our operator Q repeatedly to some initial value of the probability p. Each application of the operator corresponds to one occurrence of the response and the subsequent environmental events. The algebra involved in these manipulations is straightforward. For example, if we apply Q to p twice, we have

$$\begin{aligned} Q^2p = Q(Qp) &= a + (1 - a - b)Qp \\ &= a + (1 - a - b) \\ &\quad \times [a + (1 - a - b)p]. \end{aligned} \tag{3}$$

Moreover, it may be readily shown that if we apply Q to p successively n times, we have

$$Q^np = \frac{a}{a+b} - \left(\frac{a}{a+b} - p\right) \times (1 - a - b)^n. \tag{4}$$

Provided a and b are not both zero or both unity, the quantity $(1 - a - b)^n$ tends to an asymptotic value of zero as n increases. Therefore, Q^np approaches a limiting value of $a/(a + b)$ as n becomes large. Equation (4) then describes a curve of acquisition.

It should be noticed that the asymptotic value of the probability is not necessarily either zero or unity. For example, if $a = b$ (speaking roughly this implies that the measures of reward and work are equal), the ultimate probability of occurrence in time h of the response being studied is 0.5.

Since we have assumed that $a = 0$ when no reward is given after the response occurs, we may describe an extinction trial by a special operator E which is equivalent to our operator Q of equation (2) with a set equal to zero:

$$Ep = p - bp = (1 - b)p. \tag{5}$$

It follows directly that if we apply this operator E to p successively for n times we have

$$E^np = (1 - b)^np. \tag{6}$$

This equation then describes a curve of experimental extinction.

Probability, Latent Time, and Rate

Before the above results on continuous reinforcement and extinction can be compared with empirical results, we must first establish relationships between our probability, p, and experimental measures such as latent time and rate of responding. In order to do this, we must have a model. A simple and useful model is the one described by Estes (2). Let the activity of an organism be described by a sequence of responses which are independent of one another. (For this purpose, we consider doing "nothing" to be a response.) The probability that the response or class of responses being studied will occur first is p. Since we have already assumed that non-reinforced occurrences of other responses do not affect p, one may easily calculate the mean number of responses which will occur before the response being studied takes place. Estes (2) has presented this calculation and shown that the mean number of responses which will occur, including the one being studied, is simply $1/p$. In that derivation it was assumed that the responses were all independent of one another, *i.e.*, that transition probabilities between pairs of responses are the same for all pairs. This assumption is a bold one indeed (it is easy to think of overt responses that *cannot* follow one another), but it appears to us that any other assumption would

require a detailed specification of the many possible responses in each experimental arrangement being considered. (Miller and Frick [8] have attempted such an analysis for a particular experiment.) It is further assumed that every response requires the same amount of time, h, for its performance. The mean latent time, then, is simply h times the mean number of responses which occur on a "trial":

$$L = \frac{h}{p}. \tag{7}$$

The time, h, required for each response will depend, of course, on the organism involved and very likely upon its strength of drive or motivation.

The mean latent time, L, is expressed in terms of the probability, p, by equation (7), while this probability is given in terms of the number of trials, n, by equation (4). Hence we may obtain an expression for the mean latent time as a function of the number of trials. It turns out that this expression is identical to equation (4) of Estes' paper (2) except for differences in notation. (Estes uses T in place of our n; our use of a difference equation rather than of a differential equation gives us the term $(1 - a - b)$ instead of Estes' $e^{-\theta}$.) Estes fitted his equation to the data of Graham and Gagné (3). Our results differ from Estes' in one respect, however: the asymptotic mean latent time in Estes' model is simply h, while we obtain

$$L_\infty = h\left(\frac{a+b}{a}\right). \tag{8}$$

This equation suggests that the final mean latent time depends on the amount of reward and on the amount of required work, since we have assumed that a and b depend on those two variables, respectively. This conclusion seems to agree with the data of Grindley (4) on chicks and the data of Crespi (1) on white rats.

Since equation (7) is an expression for the mean time between the end of one response of the type being studied and the end of the next response of the type being studied, we may now calculate the mean rate of responding in a Skinner-box arrangement. If $\bar{t}$ represents the mean time required for the occurrence of n responses, measured from some arbitrary starting point, then each occurrence of the response being studied adds an increment in $\bar{t}$ as follows:

$$\frac{\Delta \bar{t}}{\Delta n} = \frac{h}{p}. \tag{9}$$

If the increments are sufficiently small, we may write them as differentials and obtain for the mean rate of responding

$$\frac{dn}{dt} = \frac{p}{h} = \omega p, \tag{10}$$

where $\omega = 1/h$. We shall call ω the "activity level" and by definition ω is the maximum rate of responding which occurs when $p = 1$ obtains.

The Free-Responding Situation

In free-responding situations, such as that in Skinner box experiments, one usually measures rate of responding or the cumulative number of responses versus time. To obtain theoretical expressions for these relations, we first obtain an expression for the probability p as a function of time. From equation (2), we see that if the response being studied occurs, the change in probability is $\Delta p = a(1 - p) - bp$. We have already assumed that if other responses occur and are not reinforced, no change in the probability of occurrence of the response being studied will ensue. Hence the expected change in probability during a time interval h is merely the change in probability times the probability p that the re-

sponse being studied occurs in that time interval:

$$\text{Expected } (\Delta p) = p\{a(1-p) - bp\}. \quad (11)$$

The expected rate of change of probability with time is then this expression divided by the time h. Writing this rate as a derivative we have

$$\frac{dp}{dt} = \omega p\{a(1-p) - bp\} \quad (12)$$

where, as already defined, $\omega = 1/h$ is the activity level. This equation is easily integrated to give p as an explicit function of time t. Since equation (10) states that the mean rate of responding, dn/dt, is ω times the probability p, we obtain after the integration

$$\frac{dn}{dt} = \frac{\omega p_0}{p_0(1+u) + [1 - p_0(1+u)]e^{-a\omega t}} = V \quad (13)$$

where we have let $u = b/a$. The initial rate of responding at $t = 0$ is $V_0 = \omega p_0$, and the final rate after a very long time t is

$$V_\infty = \left[\frac{dn}{dt}\right]_{t=\infty} = \frac{\omega}{1+u} = \frac{\omega}{1+b/a}. \quad (14)$$

Equation (13) is quite similar to the expression obtained by Estes except for our inclusion of the ratio $u = b/a$. The final rate of responding according to equation (14), increases with a and hence with the amount of reward given per response, and decreases with b and hence with the amount of work per response. These conclusions do not follow from Estes' results (2).

An expression for the cumulative number of responses during continuous reinforcement is obtained by integrating equation (13) with respect to time t. The result is

$$n = \frac{1}{1+u}\left\{\omega t + \frac{1}{a}\log\left[p_0(1+u) \times (1 - e^{-a\omega t}) + e^{-a\omega t}\right]\right\}. \quad (15)$$

As the time t becomes very large, the exponentials in equation (15) approach zero and n becomes a linear function of time. This agrees with equation (14) which says that the asymptotic rate is a constant. Both equations (13) and (15) for rate of responding and cumulative number of responses, respectively, have the same form as the analogous equations derived by Estes (2) which were fitted by him to data on a bar-pressing habit of rats. The essential difference between Estes' results and ours is the dependence, discussed above, of the final rate upon amount of work and amount of reward per trial.

We may extend our analysis to give expressions for rates and cumulative responses during extinction. Since we have assumed that $a = 0$ during extinction, we have in place of equation (12)

$$\frac{dp}{dt} = -\omega b p^2 \quad (16)$$

which when integrated for p and multiplied by ω gives

$$\frac{dm}{dt} = \frac{\omega p_e}{1 + \omega b p_e t} \quad (17)$$

where p_e is the probability at the beginning of extinction. The rate at the beginning of extinction is $V_e = \omega p_e$. Hence we may write equation (17) in the form

$$V = \frac{dm}{dt} = \frac{V_e}{1 + V_e b t}. \quad (18)$$

An integration of this equation gives for the cumulative number of extinc-

tion responses

$$m = \frac{1}{b}\log\left[1 + V_e bt\right] = \frac{1}{b}\log\left(\frac{V_e}{V}\right). \quad (19)$$

This result is similar to the empirical equation $m = K \log t$, used by Skinner in fitting experimental response curves (10). Our equation has the additional advantage of passing through the origin as it must.

It may be noted that the logarithmic character of equation (19) implies that the total number of extinction responses, m, has no upper limit. Thus, if our result is correct, and indeed if Skinner's empirical equation is correct, then there is no upper limit to the size of the "reserve" of extinction responses. For all practical purposes, however, the logarithmic variation is so slow for large values of the time t, it is justified to use some arbitrary criterion for the "completion" of extinction. We shall consider extinction to be "complete" when the mean rate of responding V has fallen to some specified value, V_f. Thus, the "total" number of extinction responses from this criterion is

$$m_T = \frac{1}{b}\log\frac{V_e}{V_f}. \quad (20)$$

We now wish to express this "total" number of extinction responses, m_T, as an explicit function of the number of preceding reinforcements, n. The only quantity in equation (20) which depends upon n is the rate, V_e, at the beginning of extinction. If we assume that this rate is equal to the rate at the end of acquisition, we have from equations (4) and (10)

$$V_e = \frac{dn}{dt} = \omega p_n = V_{\max} - (V_{\max} - V_0)(1 - a - b)^n \quad (21)$$

where we have let

$$V_{\max} = \omega\frac{a}{a + b}, \quad (22)$$

and where $V_0 = \omega p_0$ is the rate at the beginning of acquisition. If we now substitute equation (21) into equation (20), we obtain

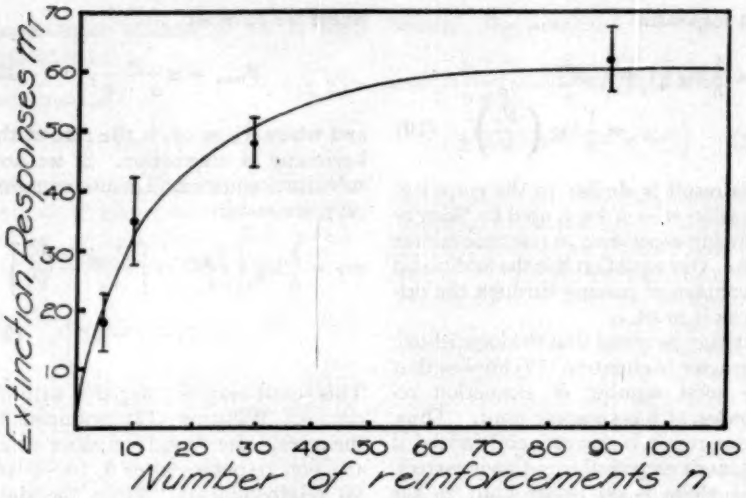
$$m_T = \frac{1}{b}\log\left\{\frac{V_{\max}}{V_f} - \left[\frac{V_{\max}}{V_f} - \frac{V_0}{V_f}\right] \times (1 - a - b)^n\right\}. \quad (23)$$

This result may be compared with the data of Williams (12) obtained by measuring the "total" number of extinction responses after 5, 10, 30 and 90 reinforcements. From the data, the ratio $V_{\max}/V_f$ was estimated to be about 5, and the ratio V_0/V_f was assumed to be about unity. Values of $a = 0.014$ and $b = 0.026$ were chosen in fitting equation (23) to the data. The result is shown in the figure.

Fixed Ratio and Random Ratio Reinforcement

In present day psychological language, the term "fixed ratio" (7) refers to the procedure of rewarding every kth response in a free-responding situation ($k = 2, 3, \cdots$). In a "random ratio" schedule, an animal is rewarded on the average after k responses but the actual number of responses per reward varies over some specified range. We shall now derive expressions for mean rates of responding and cumulative numbers of responses for these two types of reinforcement schedules. If we apply our operator Q, of equation (2), to a probability p, and then apply our operator E, of equation (5), to Qp repeatedly for $(k - 1)$ times, we obtain

$$(E^{k-1}Q)p = (1-b)^{k-1}[p + a(1-p) - bp] = p + a'(1-p) - b'p \quad (24)$$



"Total" number of extinction responses as a function of the number of reinforcements. Curve plotted from equation (23) with $b = 0.026$, $a = 0.014$, $V_{max} = 5V_0$, $V_1 = V_0$. Data from Williams (12).

where

$$a' = a(1-b)^{k-1} = a\{1-(k-1)b+\cdots\} \cong a \quad (25)$$

and

$$b' = 1-(1-b)^k = kb\left\{1-\frac{k-1}{2}b+\cdots\right\} \cong kb. \quad (26)$$

The symbol $\cong$ means "approximately equal to." In the present case the exact approach would be to retain the primes on a and b throughout; however the approximations provide a link with the previous discussion. The approximations on the right of these two equations are justified if kb is small compared to unity. Now the mean change in p *per response* will be the second and third terms of equation (24) divided by k:

$$\Delta p = \frac{a'}{k}(1-p) - \frac{b'}{k}p \cong \frac{a}{k}(1-p) - bp. \quad (27)$$

This equation is identical to our result for continuous reinforcement, except that a'/k replaces a and b'/k replaces b.

We may obtain a similar result for the "random ratio" schedule as follows: After any response, the probability that Q operates on p is $1/k$ and the probability that E operates on p is $(1 - 1/k)$. Hence the expected change in p per response is

$$\text{Expected } (\Delta p) = \frac{1}{k}Qp + (1-1/k)Ep - p. \quad (28)$$

After equations (2) and (5) are inserted and the result simplified, we obtain from equation (28)

$$\text{Expected } (\Delta p) = \frac{a}{k}(1-p) - bp. \quad (29)$$

This result is identical to the approximate result shown in equation (27) for the fixed ratio case. Since both equations (27) and (29) have the same

form as our result for the continuous reinforcement case, we may at once write for the mean rate of responding an equation identical to equation (13), except that a is replaced by a'/k. Similarly, we obtain an expression for the final rate of responding identical to equation (14) except that a is replaced by a'/k. This result is meant to apply to both fixed ratio and random ratio schedules of reinforcement.

In comparing the above result for the asymptotic rates with equation (14) for continuous reinforcement, we must be careful about equating the activity level, ω, for the three cases (continuous, fixed ratio and random ratio reinforcements). Since $1/\omega$ represents the minimum mean time between successive responses, it includes both the eating time and a "recovery time." By the latter we mean the time necessary for the animal to reorganize itself after eating and get in a position to make another bar press or key peck. In the fixed ratio case, presumably the animal learns to look for food not after each press or peck, as in the continuous case, but ideally only after every k response. Therefore both the mean eating time and the mean recovery time *per response* are less for the fixed ratio case than for the continuous case. In the random ratio case, one would expect a similar but smaller difference to occur. Hence, it seems reasonable to conclude that the activity level, ω, would be smaller for continuous reinforcement than for either fixed ratio or random ratio, and that ω would be lower for random ratio than for fixed ratio when the mean number of responses per reward was the same. Moreover, we should expect that ω would increase with the number of responses per reward, k. Even if eating time were subtracted out in all cases we should expect these arguments to apply. Without a quantitative estimate of the mean recovery time, we see no meaningful way of comparing rates of responding under continuous reinforcement with those under fixed ratio and random ratio, nor of comparing rates under different ratios (unless both ratios are large). The difficulty of comparing rates under various reinforcement schedules does not seem to be a weakness of our model, but rather a natural consequence of the experimental procedure. However, the importance of these considerations hinges upon the orders of magnitude involved, and such questions are empirical ones.

Aperiodic and Periodic Reinforcement

Many experiments of recent years were designed so that an animal was reinforced at a rate aperiodic or periodic in time (7). The usual procedure is to choose a set of time intervals, $T_1, \cdots, T_n$, which have a mean value T. Some arrangement of this set is used as the actual sequence of time intervals between rewards. The first response which occurs after one of these time intervals has elapsed is rewarded.

To analyze this situation we may consider k, the mean number of responses per reward, to be equal to the mean time interval T multiplied by the mean rate of responding:

$$k = T\frac{dn}{dt} = T\omega p. \qquad (30)$$

Equation (29) for the expected change in probability per response is still valid if we now consider k to be a variable as expressed by equation (30). Thus, the time rate of change of p is

$$\frac{dp}{dt} = \frac{a}{T}(1 - p) - \omega b p^2. \qquad (31)$$

With a little effort, this differential equation may be integrated from 0 to

t to give

$$\frac{dn}{dt} = \omega p$$

$$= \frac{\omega}{z} \frac{(s-1)+(s+1)Ke^{-\omega t/T}}{1-Ke^{-\omega t/T}} \quad (32)$$

where

$$z = 2\omega Tb/a, \quad (33)$$

$$s = \sqrt{1+2z}, \quad (34)$$

$$K = (1+zp_0-s)/(1+zp_0+s). \quad (35)$$

For arbitrarily large times t, the final rate is

$$\left(\frac{dn}{dt}\right)_{t=\infty} = \frac{\omega}{z}(s-1). \quad (36)$$

For sufficiently large values of T, z becomes large compared to unity and we may write approximately

$$\left(\frac{dn}{dt}\right)_{t=\infty} = \omega\sqrt{2/z} = \omega\sqrt{a/b\omega T}. \quad (37)$$

Thus, for large values of T, the final rate varies inversely as the square root of T.

Periodic reinforcement is a special case of aperiodic reinforcement in which the set of time intervals, $T_1, \cdots, T_n$, discussed above, consists of a single time interval, T. Thus, all the above equations apply to both periodic and aperiodic schedules. One essential difference is known, however. In the periodic case the animal can learn a time discrimination, or as is sometimes said, eating becomes a cue for not responding for a while. This seems to be an example of stimulus discrimination which we will discuss in a later paper.

Extinction After Partial Reinforcement Schedules

In the discussion of extinction in earlier sections, it may be noted that the equations for mean rates and cumulative responses depended on the previous reward training only through V_e, the mean rate at the beginning of extinction. Hence, we conclude that equations (18) and (19) apply to extinction after any type of reinforcement schedule. However, the quantities V_e and b in our equations may depend very much on the previous training. Indeed, if our model makes any sense at all, this must be the case, for "resistance" to extinction is known to be much greater after partial reinforcement training than after a continuous reinforcement schedule (7).

Since the rate at the start of extinction, V_e, is nearly equal to the rate at the end of acquisition, it will certainly depend on the type and amount of previous training. However, the logarithmic variation in equations (19) and (20) is so slow, it seems clear that empirical results demand a dependence of b on the type of reinforcement schedule which preceded extinction. We have argued that b increases with the amount of work required per response. We will now try to indicate how the required work might depend upon the type of reinforcement schedule, even though the lever pressure or key tension is the same. For continuous reinforcement, the response pattern which is learned by a pigeon, for example, involves pecking the key once, lowering its head to the food magazine, eating, raising its head, and readjusting its body in preparation for the next peck. This response pattern demands a certain amount of effort. On the other hand, the response pattern which is learned for other types of reinforcement schedules is quite different; the bird makes several key pecks before executing the rest of the pattern just described. Thus we would expect that the average work required per *key peck* is considerably less than for continuous reinforcement. This would imply that b is larger and thus "resistance" to extinction is less

for continuous reinforcement than for all other schedules. This deduction is consistent with experimental results (7). However, this is just part of the story. For one thing, it seems clear that it is easier for the organism to discriminate between continuous reinforcement and extinction; we have not handled this effect here.

Summary

A mathematical model for simple learning is presented. Changes in the probability of occurrence of a response in a small time h are described with the aid of mathematical operators. The parameters which appear in the operator equations are related to experimental variables such as the amount of reward and work. Relations between the probability and empirical measures of rate of responding and latent time are defined. Acquisition and extinction of behavior habits are discussed for the simple runway and for the Skinner box. Equations of mean latent time as a function of trial number are derived for the runway problem; equations for the mean rate of responding and cumulative numbers of responses versus time are derived for the Skinner box experiments. An attempt is made to analyze the learning process with various schedules of partial reinforcement in the Skinner type experiment. Wherever possible, the correspondence between the present model and the work of Estes (2) is pointed out.

REFERENCES

1. CRESPI, L. P. Quantitative variation of incentive and performance in the white rat. *Amer. J. Psychol.*, 1942, **55**, 467–517.
2. ESTES, W. K. Toward a statistical theory of learning. PSYCHOL. REV., 1950, **57**, 94–107.
3. GRAHAM, C., AND GAGNÉ, R. M. The acquisition, extinction, and spontaneous recovery of a conditioned operant response. *J. exp. Psychol.*, 1940, **26**, 251–280.
4. GRINDLEY, C. C. Experiments on the influence of the amount of reward on learning in young chickens. *Brit. J. Psychol.*, 1929–30, **20**, 173–180.
5. HILGARD, E. R., AND MARQUIS, D. G. *Conditioning and learning.* New York: D. Appleton-Century Co., 1940.
6. HULL, C. L. *Principles of behavior.* New York: Appleton-Century-Crofts, 1943.
7. JENKINS, W. O., AND STANLEY, J. C. Partial reinforcement: a review and critique. *Psychol. Bull.*, 1950, **47**, 193–234.
8. MILLER, G. A., AND FRICK, F. C. Statistical behavioristics and sequences of responses. PSYCHOL. REV., 1949, **56**, 311–324.
9. SEARS, R. R. Lectures at Harvard University, Summer, 1949.
10. SKINNER, B. F. *The behavior of organisms.* New York: Appleton-Century-Crofts, 1938.
11. SOLOMON, R. L. The influence of work on behavior. *Psychol. Bull.*, 1948, **45**, 1–40.
12. WILLIAMS, S. B. Resistance to extinction as a function of the number of reinforcements. *J. exp. Psychol.*, 1938, **23**, 506–521.

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AN EXPERIMENTAL INVESTIGATION OF DIFFERENTIAL SECONDARY REINFORCING EFFECTS WITH TWO DIFFERENT DRIVES

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This study was designed to answer the following question: Does secondary reinforcement, if and when it occurs, operate to produce quantitatively different effects with different drives? The two drives selected for investigation were hunger and thirst, and the process selected was experimental extinction.

The concept of secondary reinforcement occupies an important position in Hull's (11) theoretical system. Stimuli which are consistently present simultaneously with the occurrence of conditions of primary reinforcement are said to acquire properties of reinforcement themselves.[1] Since the primary gradient of reinforcement is considered by Hull to be only about 30 seconds, it can be seen how pivotal is secondary reinforcement in maintaining a somewhat extended behavior chain. Although the concept is perhaps not immediately recognizable in the characteristic sign-gestalt terminology of Tolman (22), it nevertheless does appear in a somewhat modified form when Tolman writes that "the 'sign object' becomes the 'goal object'" (22, p. 148). That this is indeed secondary reinforcement in sign-gestalt terminology is evident from the experiment which Tolman cites; this is an investigation by Williams (25), who found that animals would learn a 14-unit multiple T-maze with no incentive other than an empty food box from which they had previously been fed. In Hull's terminology this box had obviously acquired secondary reinforcing properties.

It cannot be said, however, that even within the ranks of those who might be designated as "Hullians" there is complete agreement as to the nature of secondary reinforcement. Perhaps the chief modification is that made by Spence, who suggests that possibly no primary gradient of reinforcement exists; and that, consequently, learning involving periods of delay proceeds under secondary reinforcement exclusively. To quote Spence:

> "The hypothesis suggested . . . is that all such learning occurs as a result of secondary reinforcement, the action of which is conceived to take place immediately upon the occurrence of the response" (21, p. 8).

In connection with the gradient of reinforcement it is perhaps worth mentioning in passing that in his original formulation Hull (9) regarded his goal gradient hypothesis as a primary principle; subsequently, however, by invoking the concept of secondary reinforcement, he transformed this, making it a secondary, derivable phenomenon (10).

[1] Hull's view is that "the first secondary reinforcing stimulus acquires its power of reinforcement by virtue of having conditioned to it some fractional component of the need reduction of the goal situation, whose occurrence, wherever it takes place, has a specific power of reinforcement in a degree proportionate to the intensity of that occurrence" (11, p. 100).

A number of studies have been undertaken to demonstrate the effects of secondary reinforcement; in other studies the concept has been invoked to make theoretical sense out of ostensibly contradictory phenomena. Consider, first, some of the latter studies: The failure of Warden and Haas (23) and of Watson (24) to find differences in the rate of learning in delayed and non-delayed reward is attributed by Hull to their failure to control secondary reinforcement, that is, the animals were admitted *immediately* into the food box. Differences in results between Hamilton (8) and Wolfe (26) (both of whom found learning with delays up to 20 minutes) and Perin (15), whose work constitutes the primary basis of Hull's postulation of a 30 second primary gradient, are also attributed by Hull to control of secondary reinforcement by Perin. Similarly, Denny (3) attempts to explain Humphreys' (12) demonstration that experimental extinction requires a greater number of trials when animals have been reinforced less than 100 per cent of the time on the grounds that secondary reinforcement factors were uncontrolled on the so-called non-reinforcement runs; and a somewhat similar argument is adduced by Lawrence and Miller (14) with respect to an analogous study by Finger (5).

Among studies usually cited as evidence for the existence of the phenomenon of secondary reinforcement are those of Bugelski (1) and Skinner (20), who showed that response strength was affected by the click of the Skinner box mechanism. More recently Denny (2) presented evidence in support of his hypothesis that the use of differential end boxes ought to increase learning efficiency. Since this improvement in learning is a theoretical requirement demanded by (*i.e.*, deducible from) the secondary reinforcement formulation, Denny interpreted the 20 per cent improvement in efficiency actually found as evidence for the effects of secondary reinforcement. Perkins (16), in an ingenious experimental design which aimed at eliminating the *differential* effects of secondary reinforcement by systematically shifting the arms of the T-maze, eliminating external cues, and rotating the maze, found significant differences in learning. He interpreted his results as evidence that the "extent of delay of reinforcement gradient is a function of the degree to which secondary reinforcement is operative" (16, p. 379). Following Spence, he expressed doubt about the existence of a primary gradient of reinforcement. Grice (6) presented evidence which he interpreted as "consistent with a theory which assumes no primary delay of reinforcement gradient but accounts for learning under delayed reward conditions in terms of some type of immediate secondary reinforcement" (6, p. 16). The study is of additional interest because Grice interpreted the data as evidence for the acquisition of secondary reinforcement properties by proprioceptive stimuli. Such a possibility was previously discussed by Spence in the analysis already cited (20), on the basis of experimental findings by Gulde (7) and by Riesen (17).

It must be admitted that the selection of experimental extinction as the process to be studied is not without problems; and this is perhaps particularly true in the maze situation, where there is no clear operant behavior, a circumstance which clouds the behavior with some ambiguity. Moreover, there appears to be no generally accepted criterion of extinction, and it is at least possible that the use of different criteria by different investigators may lead to apparently conflicting results. Among various criteria of extinction reported by investigators are two successive re-

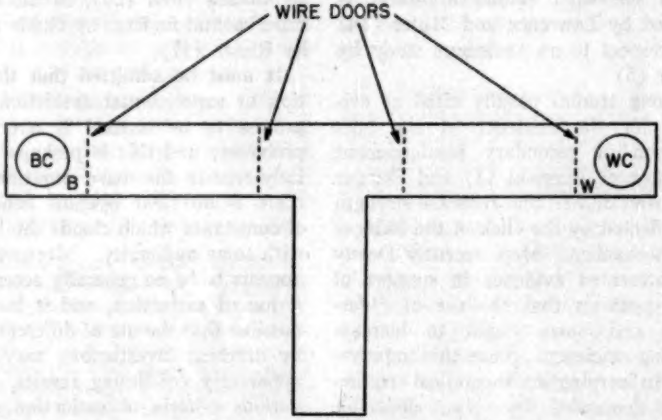
sponses to the non-preferred side of the T-maze (4), a ten-second latency of response (18), and a five-minute latency on two successive trials (14). The criterion selected for the present study was based on the probability integrals of the t distribution; it was considered that the criterion of extinction was reached when five successive runs satisfied the equation

$$\frac{k - np}{\sqrt{npq}} = t_{.25}$$

which means, of course, that any probability of t greater than .25 is also considered to be satisfactory for the extinction criterion.

The subjects used in this investigation were 32 albino rats about 90 days old which had not previously been used for experimental purposes. They were assigned at random to groups as will be indicated subsequently. The apparatus consisted of a single unit T-maze with white and black compartments as shown in the figure, 16 metal food cups painted white inside and outside, 16 metal food cups painted black, 16 white-lined food boxes, and 16 black-lined food boxes. In the preliminary training period all animals were given the opportunity to adapt to a 24-hour maintenance schedule for a week before training. Sixteen of the animals were put on a schedule of water deprivation and 16 on a schedule of food deprivation. Following this, they were run one trial a day for eight days in a straightaway, with an appropriate incentive. In the T-maze learning situation eight of the hungry animals found food in the white compartment at the right and eight found food in the black compartment at the left; eight of the thirsty animals found water in the white compartment and eight in the black compartment. Assignment to these groups was random. The metal cups containing the food and the water were appropriate to the compartment, that is, the white cups were placed in the white compartment and the black cups in the black compartment. When the animals had tasted the food or the water they were lifted from the maze and placed in the appropriately lined individual food boxes to complete feeding or drinking. The metal cups here were again appropriate.

For the extinction trials each group was divided in half. There were there-



B—black compartment. BC—black cup. WC—white cup. W—white compartment.

TABLE I

NUMBER OF RUNS REQUIRED BY EACH S TO REACH THE CRITERION OF EXTINCTION

DRIVE		Method of Extinction: S-WR S	trials	D-WL S	trials	S-BL S	trials	D-BR S	trials	
Hunger		D-4	14	C-1	24	A-3	14	D-1	22	
		E-1	14	B-1	18	B-3	19	E-4	14	
		E-3	34	C-2	14	A-2	14	D-3	14	
		C-3	14	A-4	33	B-2	11	C-4	15	
			—		—		—		—	
		T	76		89		58		65	288
		Mean	19.00		22.25		14.50		16.25	18.00
Thirst		I-1	20	G-1	31	F-3	23	I-3	14	
		I-2	15	G-2	16	G-4	14	J-3	13	
		H-3	14	F-4	25	F-2	14	I-4	14	
		J-2	14	G-3	23	F-1	14	J-4	38	
			—		—		—		—	
		T	63		95		65		79	302
		Mean	15.75		23.75		16.25		19.75	18.875
		GT	139		184		123		144	590
		Mean	17.375		23.00		15.375		18.00	

fore eight groups, which were labelled as follows:

S-WR: *i.e.*, same-white, right; these animals, which had been fed in the white compartment, now found empty food cups in that same compartment.

D-WL: *i.e.*, different-white, left; these animals, which had been fed in the black compartment at the left, now found empty *white* cups in a *white* compartment.

S-BL: *i.e.*, same-black, left; these animals, which had been fed in the black compartment at the left, now found empty black food cups in that same compartment.

D-BR: *i.e.*, different-black, right; these animals, which had been fed in the white compartment at the left, now found a *black* cup, empty, in a *black* compartment.

The experimental procedure with the thirsty animals was similar, thus providing the eight experimental groups indicated. The animals were placed in appropriate boxes containing empty food cups or empty water cups depending on the extinction group to which they were assigned. They were permitted to remain there for an hour and were then given food or water in the home cage. Failure to make a choice at the choice-point after a ten-minute period was taken as an indication that the animal had attained the extinction criterion.

The data for all eight groups (number of runs required to reach the extinction criterion) are presented in Table I. The statistical treatment applied to these data was an analysis of variance. Since this method assumes homogeneity of variance, it was first necessary to perform the Welch-Nayer L-1 test to determine the justification for such an assumption. The resulting L-1 value was .640; since this is greater than the tabled five per cent value of .520 there is no reason to reject the assumption of homogeneity, and the method of analysis of variance is permissible. The essentials of this analysis are presented in Table II. It will be immediately obvious that the variation associated with

TABLE II

ANALYSIS OF VARIANCE OF EXTINCTION TRIALS

Source of variation	Sum of squares	df	Variance
Between drives	6.125	1	6.125
Between methods	252.00	3	84.00
Within groups	1227.50	24	51.146
Residual	50.25	3	16.75
Total	1535.875	31	

drive differences is inconsequential. No F-test is needed in this case to show that there were no differential secondary reinforcing effects associated with difference in drive, and the answer to the question posed at the beginning of this paper may be said to be negative.

The test involving *method* variance is as follows: $F = 84.00 \div 51.146 = 1.642$. This F, for the indicated degrees of freedom, is not significant at the five per cent level. This was verified by performing an analysis of variance separately for the hunger and the thirst groups. The relevant data are presented in Table III. Neither of the two F's turns out to be significant.

The results of this investigation must be correlated with the recent paper by Schoenfeld, Antonitis, and Bersh (19); they have made it quite clear that the concept of secondary reinforcement and the conditions required for imparting reinforcing properties to an initially neutral stimulus require a great deal of clarification. Their study deals with operant behavior, but there seems to be no reason why their failure to secure secondary reinforcement and their analysis of the complexity of the problem should not bear some instructive lessons for maze behavior. The present investigation reveals no differential effects associated with different drives; but one is not yet permitted to conclude that the nature of the drive is irrelevant. Indeed, in the words of Schoenfeld, Antonitis, and Bersh, "it raises the general problem of identifying and specifying the conditions necessary and sufficient for establishing a neutral stimulus as a secondary reinforcer" (19, p. 40). We, however, would go further. We would raise a question about *the* conditions, that is, we would raise a question as to whether there might not be a relation between the set of conditions required for imparting the reinforcement and the method selected for *demonstrating* the reinforcement. It seems to us a gratuitous assumption to say that the necessary and sufficient conditions would be identical where the demonstration occurs in the *acquisition* of a response and in the *extinction* of one.

TABLE III

ANALYSIS OF VARIANCE OF EXTINCTION TRIALS FOR HUNGER AND THIRST GROUPS SEPARATELY

Source of variation		Sum of Squares	df	Variance
H	Between groups	137.50	3	45.83
	Within groups	582.50	12	48.54
	Total	720.00	15	
T	Between groups	164.75	3	54.98
	Within groups	645.00	12	53.75
	Total	809.75	15	

REFERENCES

1. BUGELSKI, R. Extinction with and without sub-goal reinforcement. *J. comp. Psychol.*, 1939, 26, 121–134.
2. DENNY, M. R. The effect of using differential end boxes in a simple T-maze learning situation. *J. exp. Psychol.*, 1948, 38, 245–249.
3. ——. The role of secondary reinforcement in a partial reinforcement learning situation. *J. exp. Psychol.*, 1946, 36, 373–389.
4. FARBER, I. E. Response fixation under anxiety and non-anxiety conditions. *J. exp. Psychol.*, 1948, 38, 111–131.
5. FINGER, F. W. The effect of varying conditions of reinforcement upon a [illegible]le running response. *J. exp. Psychol.*, 1942, 30, 53–68.

6. GRICE, G. R. The relation of secondary reinforcement to delayed reward in visual discrimination learning. *J. exp. Psychol.*, 1948, **38**, 1–16.
7. GULDE, C. J. The effects of delayed reward on the learning of a white-black discrimination problem by the albino rat. Unpublished Master's thesis, Univ. Iowa, 1941.
8. HAMILTON, E. L. The effect of delayed incentive on the hunger drive in the white rat. *Genet. Psychol. Monogr.*, 1929, **5**, 131–207.
9. HULL, C. L. The goal gradient hypothesis and maze learning. PSYCHOL. REV., 1932, **39**, 25–43.
10. ——. The goal gradient hypothesis applied to some 'field force' problems in the behavior of young children. PSYCHOL. REV., 1938, **45**, 271–299.
11. ——. *Principles of behavior*. New York: Appleton Century, 1943.
12. HUMPHREYS, L. G. The strength of a Thorndikian response as a function of the number of practice trials. *J. comp. Psychol.*, 1943, **35**, 101–110.
13. JOHNSON, P. O. *Statistical methods in research*. New York: Prentice-Hall, 1949.
14. LAWRENCE, D. H., AND MILLER, N. E. A positive relation between reinforcement and resistance to extinction produced by removing a source of confusion. *J. exp. Psychol.*, 1947, **37**, 494–509.
15. PERIN, C. T. A quantitative investigation of the delay-of-reinforcement gradient. *J. exp. Psychol.*, 1943, **32**, 37–51.
16. PERKINS, C. C. The relation of secondary reward to gradients of reinforcement. *J. exp. Psychol.*, 1947, **37**, 377–392.
17. RIESEN, A. H. Delayed reward in discrimination learning by chimpanzees. *Comp. Psychol. Monogr.*, 1940, **15**, 1–53.
18. ROHRER, J. H. Experimental extinction as a function of the distribution of extinction trials and response strength. *J. exp. Psychol.*, 1947, **37**, 473–493.
19. SCHOENFELD, W. N., ANTONITIS, J. J., AND BERSH, P. J. Preliminary study of training conditions necessary for secondary reinforcement. *J. exp. Psychol.*, 1950, **40**, 40–45.
20. SKINNER, B. F. *Behavior of organisms*. New York: Appleton, 1938.
21. SPENCE, K. W. Role of secondary reinforcement in delayed reward learning. PSYCHOL. REV., 1947, **54**, 1–8.
22. TOLMAN, E. C. *Purposive behavior in animals and men*. New York: Century, 1932.
23. WARDEN, C. J., AND HAAS, E. L. Effect of short intervals of delay in feeding upon speed of learning. *J. comp. Psychol.*, 1927, **7**, 107–116.
24. WATSON, J. B. Effect of delayed feeding upon learning. *Psychobiol.*, 1917, **1**, 51–60.
25. WILLIAMS, K. A. Reward value of a conditioned stimulus. *Univ. Calif. Publ. Psychol.*, 1924, **4**, 31–55.
26. WOLFE, J. B. Effect of delayed reward on learning in the white rat. *Genet. Psychol. Monogr.*, 1929, **5**, 131–207.

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PSYCHOLOGY AND THE CONCEPT OF "LIFE"

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Contemporary psychology is a naturalistic discipline. Its introductory texts are discreetly mechanistic; its advanced texts are bolder. Its journals look down their noses at animistic terminology, and its heavier thinkers hasten to assure us that there must be "neural correlates" for even their more elaborate constructs.

Naturalism is a point of view which must be defended, and the orthodox biologist has, of course, been defending it for decades. Nevertheless, there remain distinguished heretics (3, 6, 7). It is an interesting fact that the success or failure of the defense is not often a matter of great importance to the average biologist. He works with more or less isolated tissues: a muscle, a kidney, a lung. Almost everyone will grant that each of these isolates is a mechanistic system, and no controversy arises. What everyone will *not* admit is that there is no *central* vitalistic principle. The "mind" is the stronghold of vitalism, and it is for the proprietors of the mind that the question of vitalism versus mechanism assumes the greatest significance.

Presumably the ultimate defense of the mechanistic view lies in an explanation of life-processes in physicochemical terms. When the biochemist completes his job, we feel, we shall have an argument difficult to answer. But this is strictly a long-term prospect, and the mechanist must now look to less imposing bulwarks.

Among these attainable defenses we should expect to find an adequate definition of the concept of "life." The concept pretty clearly exists on a nonverbal level; there is little variability in the subject-matter of biology texts. Yet the verbalization of the concept has long eluded us. And so long as we cannot say what life is, even descriptively, so long will there be suspicion that we can *never* say, and that life can be defined only in terms of itself. The purpose of this brief note is to call attention to recent advances toward an adequate definition of life, and to the possibility that the problem of definition has actually been solved.

The new progress hinges largely on the biochemical concept of *autocatalysis*. The concept has become especially important during the last decade or two, particularly in connection with the investigation of the virus and the enzyme. As the word implies, autocatalysis is the process in which a substance acts to catalyse, from its environment, additional quantities of the substance itself. Autocatalytic action is an obvious property of the virus, and on close examination it appears indeed to be the sole claim to animation which that organism is able to advance.

Here then is a criterion which seems to differentiate the borderline cases, and there are indications that it will serve more broadly as a basic indicium of life. One authority has recently said so explicitly (5); another, Jaques Loeb, was essentially saying so many years ago (4); and the notion of autocatalysis is embodied in other sophisticated attempts to delineate the concept of life (1, 2).

This general course of thought may perhaps be integrated in formal definitions. It seems reasonable now to say that *"life" is a collective term for "living objects"*; and a *"living object" is*

an object which will absorb substances from some environment and synthesize from them a replication of each of the substances which comprise that object.

It will be noted that this formulation implies that the particular kinds of growth and reproduction often considered as criteria of life are valid ones. At the same time, by specifying synthesis, it eliminates on one hand the bothersome instances of crystalline growth and osmotic swelling and on the other such cases of pseudo-reproduction as fragmentation. By specifying absorption, it disallows the oft-cited claims of the burning flame, without, perhaps, infringing upon those of the virus. And lastly, by not insisting that living objects *do* perform as indicated, but rather only that they *will*, this formulation manages to embrace the spores, seeds, and other forms of latent life which have long been a source of difficulty.

The ordinary definition is an arbitrary thing, and it cannot be challenged. When we attempt to verbalize an existing concept, however, there arise legitimate empirical questions of validity. The present formulation must be prepared to withstand such probing. If it does so successfully, it promises to provide support for a naturalistic philosophy of psychology. The implications for pedagogy, thought, and research may then be considerable.

REFERENCES

1. ALEXANDER, J. *Life; its nature and origin.* New York: Reinhold Publishing Corporation, 1948.
2. GERARD, R. W. *Unresting cells.* New York and London: Harper and Brothers, 1940.
3. LILLIE, R. S. *General biology and philosophy of organism.* Chicago: University of Chicago Press, 1945.
4. LOEB, J. *The organism as a whole.* New York and London: G. P. Putnam's Sons, 1916.
5. PLUNKETT, C. R. The primary physicochemical basis of organic evolution. In J. Alexander (Ed.), *Colloid chemistry.* New York: Reinhold Publishing Corporation, 1944, Vol. 5.
6. SCHROEDINGER, E. *What is life?* New York: The Macmillan Company, 1945.
7. SHERRINGTON, C. *Man on his nature.* New York: The Macmillan Company, 1941.

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PERCEPTUAL DEVELOPMENT: SOME TENTATIVE HYPOTHESES [1]

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Such an abundance of new material is coming to hand from experimental psychology and clinical psychology regarding the dynamics of perception that we have—as have others (6, 61)—been interested in attempting an integration utilizing both sources. The primary conception which we have exploited (at its root not so very different from John Dewey's 1896 formulation) is that perception is a form of continuous adjustment to environmental requirements, involving not simply a "sensorium" or seat of cognitive functions, but the whole organism. But from this basic idea, empirical facts and personal idiosyncrasies may lead speculation in any one of many directions; and the one which has especially interested us is the conception of perceptual learning, the conception that the whole organism, in coping with its environment, progressively alters its modes of perception and develops more and more complex ones that serve it better. What we are trying to say will be presented under a series of eighteen hypotheses. It would be good if these hypotheses could be prepared in the form of postulates, but they are neither as clearly thought through nor as coherent as are the usual postulates of science, and on the other hand some of them take the form of propositions regarding which a good deal of empirical material has been assembled. They are presented here, however, neither as substantiated principles nor as original and unfounded theses, but rather as a very tentative system of ideas to be worn "like a loose garment," a makeshift which may be replaced by more appropriate garments when the facts are clearer and their interrelations more evident.

[1] The main outlines of this paper were presented by the senior author on June 7, 1949, to the British Psychological Society in London. The present form of the paper represents both authors' views equally, not by compromise but by coalescence.

The Hypotheses

(1) *Perception is typically, if not always, both unitary and structured*—a *unitas multiplex*. The tree-frog's piping or the roll of drums gives tonal elements, but elements in temporal order and with a quality which reflects the whole context of the hearing. This first hypothesis involves the recognition of the difference between an assemblage of parts and an organized structure, and also the conception that, in this whole, differentiable aspects can still be observed. There are phenomenal aspects which can be noted separately although they are held in a sort of central coherence: such "parts" (objects, extended forms, etc.) display considerable stability and separable identity insofar as they are of defined extent, and can with relative independence be shuffled and manipulated; thus, a leaf or a robin in the visual field may change its spatial location, size, orientation, and may even

be partially occluded by other objects and still maintain its individual identity and form (23). However (as is attested by their constancy in the face of changing stimulation), the existence and the characteristics of such spatially delimited "parts" cannot be ascribed to the action of any comparably separable or spatially delimited aspects of the pattern of stimulus excitation (*i.e.*, perceived size is not a univocal function of visual angle, or retinal shape, or retinal stimulus intensity, etc.). Instead, the characteristics of any phenomenal "part" depend upon relationships within the total *extended* field of impinging stimulation, and upon many diverse and, frequently, mutually reinforcing (8) aspects of the stimulus distribution.[2]

(2) *A primordial aspect of the structure of the percept is differentiation between figure and ground.* Here are emphasized facts such as the following: that the phenomenal content of visual perception consists of shaped regions and relatively unshaped ground, and not of immutable fixed elements such as mosaic points; and that accent, or anchorage, is always well marked. There may be a moment of instability, when wood-thrush song is melted into "forest murmurings," but figure-ground relations are quickly established, and any changes in organization must either occur within the relationships first laid down or pass through only transitory and limited periods of ambiguous dominance. This figure-ground differentiation exhibits greater or lesser degrees of sharpness (62). It is not a single bipolar process, with one figure stably superposed upon one ground. Rather, it is capable of extreme complication (23) —as the listener at *Parsifal* finds it hard to "take it all in"—especially as we leave the static fixation of the laboratory for the free glance and the moving observer of more natural perceptual conditions (56).

(3) *Structure depends genetically upon two processes:* (a) *differentiation within a more-or-less homogeneous matrix, and* (b) *integrations of the differentiated phases.* Here reference is made to Herbert Spencer's conception of progress from homogeneous to heterogeneous; likewise, to the evidence of Werner, Piaget, and others that diffuseness (and even a "syncretic" lack of differentiation between self and environment) is characteristic of the child's earliest percepts, and that the development of parts can actually be observed as the child takes hold of, points to, and names specific aspects, while other aspects of the environment remain "blurred" or completely undifferentiated. The elephant may stand out in the child's apprehension of the circus—just as the elephant in the circus poster stands out against the painted jungle behind.

Integration of the differentiated phases comes only as a result of differentiation and only as interaction occurs between the differentiated phases. Thus, in perceiving words as compounds of letters, it is at first necessary to differentiate the letters (which are not, in general, sufficiently "good" forms to insure such discrimination on autochthonous grounds alone), and then to integrate those letters into larger, characteristic word-units (the modern teacher may teach words before letters; this means simply postponing, until the child is ready, the analytical learning of letters).

The same processes of perceptual development can be observed in the adult by presenting new "micro-scales" (60), *within* the musical intervals now in use, in which the initial indiscriminability

[2] Thus, the perceived brightness of a particular region depends upon the distribution of stimulation in other regions (58); upon preceding conditions (18) and "memory" (12); upon stimulation in other modalities (52); upon the extremely complex relationships of contours, proprioception, *brightness itself*, etc. which determine perceived space (20), etc.

of the "notes" gives way to ready perception of tones which still later integrate to form "micro-melodies" (it is probably in such culturally unstressed areas, as tactile and olfactory perception, that the most fruitful investigations of perceptual development will be made); children presumably acquire our normal musical scales in like fashion.

The assumption of integration as a process which may at least tentatively be considered as separable from differentiation is supported by the inadequate integration of already-differentiated parts (evidenced by relatively independent spatial lability) found in children (60); by those cases of visual agnosia (44) in which the parts of complex presentations are perceived but not the interrelations between them; by the drawings of primitives in which ". . . particulars often appear as self-subsistent things which do not necessarily become synthesized into larger entities" (60, p. 139).

Progression from (*a*) homogeneous through (*b*) differentiated to (*c*) integrated characterizes, in principle, not only the stages through which the perceptual development of the maturing child passes, but also (with important qualifications later discussed) the course of every perceptual process, regardless of age. (*a*) We hear Japanese spoken —a melodious blur; (*b*) we learn a few words and pick them out as we listen; (*c*) we follow a conversation. The initial response even in the adult is diffuse and undifferentiated. It may be incompletely separated from the "ego," unlocalized in space and time, undifferentiated as to form and modality; and it may contain strong affective and conative as well as cognitive components (5, 9). Although the course of such development is, with familiar stimulus configurations, altered by prior differentiations and integrations, and although the durations of these stages are in most cases much shorter in the adult than in the child (perhaps simply because of his greater experience, and quite aside from the matter of neural maturation), the basic nature of the process is assumed to be the same.[3]

It is not implied here that the stimulus situation automatically determines the development of any percept, since there are usually many ways in which boundaries can be cut across any inhomogeneous field, and a large number of ways in which such boundaries can serve as figural contours. Admittedly, in some limiting cases the autochthonous forces of organization restrict perceptual development to configurations of highest stability (23, pp. 106–210); with increased stimulus complexity, however, the number of almost equally "good" organizations may increase, *i.e.*, in terms of stimulus considerations alone the situation may become ambiguous and any number of organizations equally possible.

(4) ***The phenomenal aspects of the resulting integration are interdependent and exhibit membership character in the whole.*** The fear in a child's voice is fully apprehended only in a context of perceiving the child and his situation. Whatever *ultimate* independence may characterize receptor processes or afferent conduction, the percept as we actually know it exhibits interdependence of phenomenal phases, and no phase can be brought into relation with a punctiform stimulation whether of the periphery or of the cortex.[4]

[3] It is not here implied that tachistoscopic exposure achieves a corresponding fractionation of percept development. Reduced exposure time probably does little more than reduce the efficacy of the stimulus.

[4] It *may* be possible to recognize a relative independence of sensory-neurone processes preceding their convergence at central levels; however, evidence such as that of retinal field-

Assuming interrelatedness of the physiological processes underlying the interrelated psychological ones, and recalling that even the most spatially delimited perceptual "part" depends for its existence and characteristics upon the total extended field of stimulation, we should expect that the *integration* of delimited "parts" involves likewise the *integration or mutual adjustment* of the extended processes upon which they depend. Thus, the whole field of physiological processes is altered in perceptual integration, and each integrated part is conditioned by—is "keyed" to—the whole field (*cf.* 23, p. 554).

(5) *Exteroceptive, interoceptive, and proprioceptive components enter fully and on an equal footing, but not always with equal weight, into the dynamics of perception.* Those sense organs which make contact with the outer world, those which make contact with the vital organs, and those which make contact with our own muscular adjustments to our environment pool their sensory contributions. Rejecting the view that perception is necessarily heavily loaded with exteroceptive components and that it therefore "mediates reality," it is here urged that it is simply the prejudice of psychologists to emphasize the distance receptors to an extraordinary degree and, therefore, to build our whole theory of perception upon a somewhat warped notion of how the organism works. The spatial world in which we live is *not* constituted by exteroception alone. In the very grossest sense, the proprioceptive pattern provides an important base to perception (anatomically, the sensory and motor functions of the nervous systems are extremely intimate, if not inextricably interrelated [13]). We observe actively, muscularly, with head, eyes, neck, and much else besides. Visual information must be pooled with proprioceptive information as to the position and motion of the receptor surfaces if effective contact with the outside world is to be maintained, and it is the excellent agreement of the percepts based upon these *two* factors with the world as we know it from other grounds which leads to the frequent neglect of the obvious inadequacy of either, and to the acceptance of a largely mythical version of visual exteroception as the basis of perception. The term "mythical" is used because the information yielded by any momentary fixation (devoid of scanning, of motion parallax, and clearly articulated only in the foveal region) bears little resemblance to the "visual" percepts upon which we tend to base not only our normal activities but (in considering the "stimuli" to which an organism responds) our scientific analyses as well. It may be possible, of course, to restrict the term "perception" to that process which occurs during such momentary fixations (although this raises the question as to where such fractionation should cease), but this course makes most of what we normally call perception the province of "judgment," "inference," "memory," etc.

Although perception is based, in part, upon both proprioceptive and exteroceptive "information," *both* components are in large part "silent" in the formation of the visual percept. We are no more directly aware of the continually fleeting and changing transformations

effects paralleling the phenomenal field-relations (39), etc., makes doubtful isolation even at peripheral levels. Be that as it may, we can here admit of no trace of punctiform sensation in perception. Although the belief probably still exists that it is possible under special attitudes to inspect one's sensations, such attempts generally reveal only some reduction of phenomenal constancy, explicable as a perceptual reorganization which rotates the region considered into a plane frontal-parallel to the observer, thus yielding percepts similar *in some ways* to the physical image on the retina.

and patterns of retinal stimulation than we are of the proprioceptive stimulation occasioned by physiological nystagmus, by the acceleration of gravity, and by the relative positions of eye, head, and trunk. It is only in special cases that the influence of one or the other becomes evident, as in situations where either component is manipulated in an "artificial" manner (*i.e.*, without reciprocal or corresponding change of the other) or in situations in which careful analysis of the psychophysical relationships reveals systematic discrepancies from what would be expected by exteroceptive components alone. Numerous studies dealing with the effects of the distribution of muscular tension on the perception of visual stimuli, and of exteroceptive stimulation on the perception of muscular tension, are summarized by Werner and Wapner (61), who conclude that perception is the product of a fusion of dynamically equivalent sensory and tonic factors—a viewpoint close to the one here accepted.

It is here urged that the same approach can be applied to the interoceptive excitation-patterns which yield "information" as to the state of our internal organs and our tissue needs. Unless we postulate an isolation between the exteroceptive and proprioceptive portions of the nervous system, on the one hand, and the interoceptive tracts, on the other, we must expect some interrelationship. Such isolation appears improbable (22); and even if anatomical isolation were complete, relationship would be expected through proprioceptive changes due to autonomic activity. It is therefore assumed that our "visual" percepts are based upon information delivered by all afferent tracts, whether they be exteroceptive, proprioceptive, or interoceptive in origin. Moreover, any effector excitation pattern aroused by those cortical excitations correlated with a percept will, through consequent muscular and visceral effects, feed back through exteroceptive, proprioceptive, and interoceptive tracts to modify the perceptual organization of the preceding moment. Thus, our conceptions of the external world are in part a product of what goes on inside us, and our conceptions of what goes on inside us are to some degree loaded by our responses to the external world.

Consonant with the view of perceptual development, it is assumed that in the most primitive forms of perception all three sources of sensory information are present in an undifferentiated or partially differentiated state and that subsequent differentiation gradually achieves more and more separation of these factors. We would expect that in primitive stages, perception of environmental objects would prove inextricably fused with the motion, position, and feeling state of the observer. Considerable evidence that the behavior of very young children, of the drug-intoxicated, and of brain-damaged patients can best be understood as the effects of such undifferentiated fusion of sensorial, affective, and motor phenomena has been collected and compellingly presented by Werner (60). The hammer is not perceived as a visual object only; it is directly perceived as heavy, and as hard, and as "a thing you strike with." In children before such areas are well differentiated, in cases of brain injury involving considerable dedifferentiation, and in schizophrenics in whom ego-object polarity is lost, it is difficult or impossible to get reactions in terms of such highly static, differentiated abstractions as objects-in-isolation or shapes-as-such. The adult separation between subject and object may be viewed as being as much a product of differentiation as is the distinction between modalities, which is likewise de-

creased in children, in patients suffering from drug-intoxication, etc., and is not always perfect even in normal adults (52). Even in the sophisticated adult, such fusion of contributions from all sources provides the basis of perception: the initial stages of perception are, like the earliest responses of childhood, a fusion of external and internal information. Undifferentiated affective, conative "pre-sensations" not specifically localized in space or time are not completely differentiated as to modality (5).

After differentiation is completed, the evidence of interaction is more difficult to come by. The very structure of our language is designed to make almost impossible a report on process or context rather than static and abstract identity (24), and in the laboratory an added effort has been made to rule out the "subjective" variable presumably so removed from science. Even in the laboratory, however, one no more normally perceives objects completely outside of any affective and conative setting than one perceives them without spatial characteristics. Such affective characteristics may be attributed to the perceived object, but they probably tend to shift, with development, towards a subjective tension or need attributed to the observer's perceived self, in line with the general developmental tendency towards stabilization of environmental objects and abstraction from their context.[5]

[5] Similar relationships appear in the cooperation of exteroception and proprioception, *e.g.*, in the way in which motion, if perceived in the object, may not at the same time be ascribed to the self and vice versa (57); in the same fashion, the affective components perceived as characteristics of the object may not at the same time be perceived simply as information concerning subject's needs—a stage of incomplete differentiation very similar to phenomena subsumed under the concept of "projection."

It is suggested, then, that as the proprioceptive components may contribute "silently" to our perception of spatially localized objects, the information from the vital organs may in most cases also be silent, yet contribute heavily, together with proprioception and exteroception, to those relationships (between the perceived self and perceptual objects) which may be subsumed under Tolman's concept of "sign-gestalten" (55), those action-impelling attributes of the perceived world which, in order to achieve "objectivity," we so rigorously train ourselves to ignore. After dinner, not before, is the time to present our case. Much of what we term "affection" would then consist (at least originally) in the contribution of interoception to the exteroceptive-proprioceptive pool; similarly, much of what we term "conation" depends upon the proprioceptive information contributed. There is, however, no warrant for identifying cognition solely with exteroceptive processes, *e.g.*, with visual, tactual, auditory sensations, and excluding organic sensations; and there is no warrant for relating affection solely to interoception, nor for relating conation solely to proprioception.

(6) *It is meaningless to separate sharply the cognitive, affective, and conative aspects of perception.* A process must be cognitive if it is to be called perception, but it may be affective and conative as well. In the normal intact organism retaining interoceptive and proprioceptive neural components, it would not only be difficult to get the exteroceptors to work at the same time that one prevented the interoceptors and proprioceptors from working; it would also be improbable that the resulting process would be anything like perception as we know it. Indeed, the terms "cognition," "affection," and "conation" become matters for phenomenological investigation rather than psychological

categories. The fact that a particular phenomenal region within the field carries the "flavor" of objectivity and cognition, and another region carries those of affection or conation, does not necessitate ascribing to them separable sensory origins. We cannot identify cognition with exteroception, affection with interoception, etc., since we have seen that cognition itself *includes* necessary interoceptive and proprioceptive elements, and we cannot talk of perception as *modified* by conative or affective factors, any more than we can talk of perception as modified by kinesthesis. Cognition, affection, and conation are therefore independent only in the sense of a necessary and sometimes rather laborious scientific task of abstraction. Perception as such can (and, moreover, must) be loaded or weighted with any one of these three processes and still remain perception.

This interdependence may perhaps also throw light on the problem of motive or "drive." Interoceptive processes, whether we are aware of them or not, are of course important aspects of many drives, *e.g.*, hunger. Hunger, while physiologically a response to tissue needs, may at the same time be just as sensory as is a patch of color. Proprioceptive processes, such as restlessness, or the impulse to change from one posture to another, likewise have a drive quality. Frequently the drive towards a goal-satisfying object, like apple pie, involves both interoceptive and proprioceptive (goal-seeking) components. Impulses from such interoceptive and proprioceptive sources may fuse freely with exteroceptive sources, and as the apple pie in the kitchen is smelled or seen, we do not merely perceive it; we perceive it hungrily. Hunger does not necessarily becloud the cognitive situation, for the flaky crust, the spicy aroma, are more vividly apprehended than when we are neutral toward such things. From such a homely example of the fusion of exteroceptive, interoceptive, proprioceptive components we may proceed by gradations to Robert Burns's "for to see her was to love her" and to Spinoza's "intellectual love of God."

To become aware of our drives, in contradistinction to becoming aware of the things we want, is usually a form of perception in which the interoceptive processes receive emphasis; but all drives, insofar as we are aware of them at all, become, from this viewpoint, *perceptual phenomena*. There is no need for two independent categories in our basic psychology of immediate experience, one for perception, one for drive. Both perception and drive are cognitive; both are affective; both are conative. It is just a question of convenient scientific abstraction and emphasis.

It may be asked whether, as we listlessly loll on the riverbank and eye the clouds, we perceive in the same affective and conative fashion. In reply, we suppose that if we became so perfectly neutral to the summer's day as to be free of any desire to drink it lusciously in, the interoceptive and proprioceptive components which we call drowsiness would dominate the situation. We should conclude by perceiving it neither conatively nor, indeed, in any other way than by going to sleep.

(7) *Perception develops through experience as well as through neural maturation.* It was suggested earlier that there is systematic perceptual "growth" dependent upon a general developmental principle of differentiation and integration. It seems likely, however, that experience also results in this kind of development, and that growth may be to some extent subsumable under differentiation and integration; *i.e.*, the high degree of perceptual structure characteristic of the adult may be a direct consequence of the child's successful passage through the developmental se-

quence of differentiation and integration in a large variety of stimulus conditions. The members of different cultural groups have learned to perceive as culture requires. Certainly the present evidence (48) as to the roles of use and experience would make it unwise to set limits to the relative importance of these factors in determining the perceptual repertory of the organism. On this assumption, individual differences in experience, in interaction with the environment, will alter the rate and form of perceptual development. Environments differ in the direction and extent to which they force us to make distinctions and then to integrate what has been distinguished (it was for this reason that it was previously implied that experiments with adult human beings on percept development would not yield simple developmental laws uncontaminated by the effects of past experience [15]).

Cognition in a given situation is a function not only of the pattern of stimulation, but of the "schema," framework, or matrix within which the cognitive contact is made, as has been demonstrated by the work of Sherif (54) and Bartlett (4), and by the many experiments in perceptual "set" (10, 29, 45). If we assume that the matrix within which each percept develops may itself be so altered through experience that it guides in an altered fashion the direction and extent of the growth of the percept, then, although the initial developmental phase of each percept in the adult may be as undifferentiated as that of the child, subsequent differentiation will occur in directions previously found successful or satisfying if such growth is possible within the limits set by the stimulus distribution. If the stimulus distribution "blocks" the growth of the percept at any stage, either the development will have to take a new course within the framework or, if no successful resolution is possible, an altered or new matrix ("cued," perhaps, by the preceding perceptual development) will be brought to bear. The successive mutual adjustment of matrix, stimulus distribution, and percept in the attempt to mesh into a coherent, stable organization may *appear*, at least superficially, like trial-and-error behavior. However, where the matrix and the limits imposed by the stimulus coincide; where stimulus distribution is strong and simple, allowing of only one organization of overwhelmingly superior stability; or where the matrix is relatively strong and the stimulus distribution weak or complex and permissive of many alternative organizations, there should be no "trial-and-error" but, rather, a smooth perceptual development.

Moreover, the point at which further articulation ceases should depend not only upon what, in the abstract, constitutes a configuration of maximum autochthonous stability but also upon the demands of the situation and upon the extent to which differentiation has progressed in the past. The level at which the tourist admires masses of foliage involves far less articulation than that which the native, searching for camouflaged game, must employ; on the other hand, identifying simple geometrical forms may prove a more difficult task for the jungle native than for the tourist, to whom such "signals" are a matter of daily importance (41).

(8) *It is possible to control experimentally the phenomenal aspects of the genetic process, i.e., to teach the subject how to perceive.* Although it is granted that the sensory neurones feed specific stimulus-determined impulses to the brain, and that the basic "autochthonous" laws of organization described by Gestalt psychology impose further conditions upon what can be perceived, repeated experience with particular attributes of the environment builds up,

first, different types of differentiation and then different types of coherence between the differentiated "parts."

Consider the vast difference in the perception of a complicated electronic gadget by one who has built it and by someone else who, with knowledge neither of the gadget nor of electronics, reports a mass of colored spaghetti with anchorage points perhaps determined by brightness of color, the purely "autochthonous" simplicity of form, etc. Learning to read must certainly impose new grouping upon the perception of material printed in the appropriate alphabet; indeed, the individual letters are apparently now perceived more readily under reduced stimulus conditions than are their mirror images (19), which are presumably equivalent configurationally. Insofar as individuals have learned to make distinctions and to interrelate those distinctions in ways which serve their needs, they must perceive in a manner unlike those with different experiences and with different needs.

Of special interest are examples of "interference" with the factors which determine the figure-ground differentiation: Schafer and Murphy (53) rewarded their subjects along with the perception of certain patterns, punished them with others; later, with both stimuli presented simultaneously, figure and ground consistently followed the direction of reward and punishment. Subjects failed to see in these combined presentations the materials which had been seen just before they were punished, and saw in the presentation what they had seen earlier, just before they were rewarded. Apparently, they literally learned to perceive one way rather than another.[6] Proshansky and Murphy, (47) by giving arbitrary rewards after the presentation of long lines and punishment after short ones, caused increased estimates of line length; by similar procedure, they also produced a tendency to underestimate lifted weights. Another interesting example can be found in an experiment by Krechevsky (26) in which the autochthonous perceptual "law of proximity" was found operative in the rat only when reward-achievement was helped by such operation. The process of learning by reward and punishment appears, then, to apply to perceptual development—perhaps to the same extent and subject to the same limitations that appear in any generalization about learning.

(9) ***Repeated stimulation alone, without either known satisfaction or known frustration, also leads directly to analysis and to synthesis, i.e., differentiation and integration.*** The same passage from homogeneity to a differentiated, and from a differentiated to an integrated, level is likely to be obtained by sheer repetition without involving neural growth in the usual maturational sense and without involving satisfaction or frustration of any known nature.[7] Here we can appeal to everyday experience on the stabilization of perception; to Rubin's demonstration (51) that familiarity can determine which of two alternate figures will be seen; to a follow-up study by Schafer (53, p. 341), in which this kind of stabilization of perception occurred without external rewards and punishment; to Leeper's study on the organization of incomplete figures (32);[8] to Hanawalt's findings (16) that in-

[6] Recent repetition by I. Rock and F. S. Fleck (49) has failed to duplicate the results of this experiment. It is difficult to accept their conditions as comparable inasmuch as the design of the experiment was somewhat altered. Further work is certainly needed. (It seems likely, also, that the motivation afforded by rewards of four cents has probably changed both qualitatively and quantitatively since 1943.)

[7] Hebb (17) has recently proposed a neurological model involving something very much like differentiation and integration through sheer repetition.

creased recognition of concealed figures, as a result of practice, was at least in part dependent upon the "unessential" parts of the concealing design becoming ground; and to a striking example of the effects of sheer experience in the recently reported work of Wallach and O'Connell (59) showing that perceiving the projection of a wire figure on a flat surface as the projection of a three-dimensional structure depended upon specific past experience with the projection of that form while in rotation.

It would seem reasonable, here, to assume the operation of something very much like Woodworth's "drive to perceive" (66), *i.e.*, that even in the absence of *external* pressures, the integration of the main "anchorage points" into a coherent, stable unit is an end in itself, an assumption in close accord with the Gestalt concepts. "Sheer repetition" might then serve the function of providing opportunity for the operation of this process.[8]

(10) ***Integration tends to achieve homeostasis or stability and offers maximal resistance to change.*** Integration involves a decrease in the freedom of each differentiated part, with mutual support and reinforcement of such parts by each other and by the whole (23). This reduction of freedom may tentatively be assumed as due, at least in part, to bringing the configuration of cohesive field forces (43) to the stable state of lowest energy (3); that is, once integrated, the percept would resist change. (It would be interesting to explore the problem of whether the achievement of stability is related to rigidity as it is known to psychiatrists and clinical psychologists. Perhaps the contemporary study of feed-back systems will ultimately show that the maintenance of stability in a perceptual system is formally analogous both to the physiological maintenance of homeostatic balance (27) and to psychopathic rigidity.)

(11) ***"Association by contiguity" results directly from such synthesis.*** Something phenotypically like "association by contiguity" must occur if integration is a direct result of repeated or extended presentations of a stimulus distribution. The *integration* into a stable percept of the various simultaneously present differentiated "parts" must change the components so that they now bear membership character in the same organization (38), and each one, no longer independent, "leads to" the others through their mutual relationships with the whole. This, of course, is not association by contiguity in the usual sense inasmuch as the "elements" do not maintain their identity and inasmuch as "association" is not by addition of elements but by the modification of each part by its role in the original percept. In a sense, the "part" now bears the "keyed" imprint of the distribution of stresses in the original percept, and separate presentation of that part therefore tends to re-establish something which may be close to the percept aroused under the original conditions of stimulation. Thus, with the perception of such a "keyed" part the development of the rest of the field is guided along those paths (where permitted by the stimulus material) of differentiation and integration traversed by the original percept. This "leading-to" based upon previous structure (21) would constitute at least a large part of the matrix within which perception develops, although we should expect not the ideal operation of such tendencies under stationary stimulus distributions

[8] Perhaps perception passes through substages of *relatively* stable organization, depending either upon temporal processes (as are ambiguous figures [25]) or upon repetition under slightly altered circumstances, to be "jogged" out of the substages and to achieve maximal integration.

but, rather, continually changing, temporally-extended interrelations of partially aroused and incomplete frameworks, shifting stimulus distributions, and the feed-back from the developing percept to both.

Matrices may affect not only perceptual selection but reintegration, or closure, as well: an evoked matrix directs the development of the percept toward recreation of the original integration; and where the stimulus situation is close to that which evoked the original percept, this must result in what appears to be a "lowered threshold." Even poorly defined or incomplete fragments of previously well-integrated wholes should lead to the reintegration of something close to that whole. Thus, with impoverished and even fragmentary stimulation, we would expect lower thresholds for reporting familiar forms than for configurationally equivalent unfamiliar forms. The data reported by Henle (19), who found lower thresholds for letters than for their mirror images; by Duncker (12), who found evidence of a "memory color" evoked by shape, etc.; by Postman and Bruner (45), who found closure of incomplete circles attributable to an experimentally induced "set" to see circles; and, possibly, by Postman, Bruner, and McGinnies (46), who found lower tachistoscopic thresholds for words most consonant with "personal values," may be called upon to support this hypothesis. Such effects presumably require that the stimulus distributions permit such organization. Thus, Duncker found that memory color may increase a given hue, not create one completely absent from the stimulus; and Djang (11) found that past experience with forms affects their perception when embedded in other forms only if the autochthonous organization of the latter is not determinate, permitting alternate organization, etc.

We would expect that such re-constituted aspects of the original percept might have effects as though actually sensorially present (28). Perhaps the "conditioned response" may come under this principle, in the sense that from appropriate excitations there arise behaviors appropriate to a stimulus (or to expectation [55, 66] of a stimulus) which is not sensorially present (14) but which is part of the integrated, temporally extended world-of-action with which the organism is faced.[9]

Guidance by such matrices in no way guarantees the *validity* of the percepts, or of the behavior thereby aroused: the completion of autochthonously strong figures, or of familiar ones (7), may very well involve erroneous perception of the environment, and behavior directed toward a strongly expected event may prove unrealistic. Far away, we recognize a friend by his walk; but on closer view the walk attaches itself to an alien form—and turns out to be not quite the walk we knew.

(12) *The processes of autistic perception are only relatively distinct from realistic or objective perception insofar as the phenomenal content of a percept is concerned.* The extent to which such perceptual development deviates from what would be expected from the environment, *as we know it on other grounds*, constitutes "perceptual distortion." Where the percept is "distorted" toward an environment more consonant with the observer's "needs," such distortion is called *autism*.[10] However, *all*

[9] It is probable that, as Koffka argued (23), any hypothesis of this nature must assume the existence of memory traces; however, it does not seem essential at this stage to make explicit assumptions as to their characteristics.

[10] "Needs," especially in human adults, may often be less directly related to metabolic deprivations than to the highly complex "ego-tensions" concerning which we can learn much more from the personologist than from the physiologist; *e.g.*, the ego-mechanism may in-

perception involves adjustment between matrix and stimulus distribution; whether or not such adjustment "reconstitutes" the environment, the process remains fundamentally the same. It is, of course, perfectly possible to separate autistic and realistic processes if one refers to the degree of success in "adapting" to the external environment, or to performing certain tasks according to certain criteria of the experimenter. It is evident, however, that if we do this, we are emphasizing external criteria of success or failure, not actually describing a perceptual process. Indeed, if the environment changes somewhat while retaining certain identifiable features, the same perceptual response may be "realistic" today and "autistic" tomorrow. The man who sees at a lower tachistoscopic threshold those words which fit his "value system" (46) is only displaying autism if the stimulus distributions around which his percepts developed actually constitute other words, but the perceptual process remains the same in either case. Realism and autism are functional terms, not terms which relate to the content of experience.

(13) *By varying experimentally the exteroceptive, interoceptive, and proprioceptive components which affect perception, one may define a full continuum from realism to autism.* The experimenter may reduce one and increase another component, and he may alter their balance so as to increase realism or autism or to get determination to a varying extent by the three components considered. Sometimes the percept is almost completely exteroceptively predetermined: with well-articulated visual stimulus situations, a proprioceptive excitation pattern can be one which would, alone, result in the perception of the vertical, for example, considerably different from that indicated by vision, yet the perceived vertical will, for most observers, remain that of the exteroceptive pattern (65). Vary, however, the strength of the proprioceptive stimulation relative to that of the exteroceptive stimulation (or the extent to which the individual habitually relies upon one or the other [64]), and the perceived vertical apparently may assume compromise positions anywhere between those dictated by the proprioceptive and by the exteroceptive excitation (*cf.* 2, 34, 42). Altering the balance of interoceptive and exteroceptive components will probably likewise alter the closeness with which the response is anchored to the external stimulus situation.

As a first approximation, we may assume that increased domination of perception by nonexteroceptive components can be achieved by increasing the intensity of the nonexteroceptive components and decreasing the effectiveness of the exteroceptive determinants. The latter may be achieved by any method which increases the ease with which alternative figural organizations can be fitted to the exteroceptive stimulation. Such situations often occur before differentiation is complete (when alternative boundaries may be set up), and after bounded regions have separated but before they have been integrated into a stable structure. The probability of meeting these conditions can be increased by (*a*) the use of any method which impairs structuring of the exteroceptive pattern, whether it be by the use of short exposure (46), low illuminations (50), or decreased clarity of the stimulus-inhomogeneities upon which contours may form (as by the use of ground-glass diffusing screens [33]); or (*b*) the presentation of complex unfamiliar patterns (32) or material yielding unstable (*i.e.*, of more than one

clude ways of coping with threat which, phenotypically, may be revealed by distortions apparently aimed at perceiving that which the organism fears.

equally "good" alternative structure) or potentially reversible figure-ground differentiation (53). These are the situations most explored in studies of autism, perceptual defense, and in projective techniques.

(14) *Satisfying aspects of percepts tend to become figure in a progressively more focused manner.* The organism is often sensitive to several goals at once, its neuromuscular system being incompletely focused, but it becomes more and more focused or set toward those specific goals which have in the past proved satisfying (figure-ground segregation is not necessarily all-or-none but may be a matter of degree).

In terms of the hypotheses presented, with perception a temporally-extended developmental process, integration must involve the bringing-together of temporally separated parts (if only through the interaction between those parts which overlap temporally). The extent to which such integration occurs determines the extent to which each part is modified so as to "lead" to the future pattern (as a few notes from a melody lead to its completion) as well as contemporary "keyed" parts.

Where such integration—or structural resolution—depends *chiefly* upon the limitations set by the exteroceptive stimulus conditions we may, if we wish, refer the resultant integration to the reinforcement of a drive-to-perceive, as does Woodworth (66), or to the achievement of a configuration of maximum stability, as would the Gestaltists. Where integration awaits the resolution of interoceptive components aroused by tissue needs, the perceptual integration of greater stability will usually be the one that generates actual behaviors which reduce the tissue need (33). Upon rearousal of perceptual parts to which such interoceptive information has contributed, those parts will "lead to" the completed perceptual integration and, therefore, indirectly to that kind of action which relieves the needs. Here, as before, we can refer either to the concept of reinforcement or to the achievement of stable perceptual configurations and still be referring to the same process; *from this viewpoint the controversy regarding the Law of Effect is purely terminological.*

Thus, the strength of different needs with which the individual is faced and the history of their satisfaction or their frustration become fundamental in the determination of any characteristic differences in personal outlook.

(15) *The trend toward integration tends to be irreversible.* This hypothesis is certainly both controversial and far from capable of full verification. It has been assumed (p. 333) that progressive differentiation tends to occur *within* the major boundaries already laid down, and we know (23) that those regions of the field having the greatest internal articulation tend toward figural status; since integration occurs at each successive level, reversal, in the sense of eliminating boundaries or contours already in existence and creating new ones, would be difficult. Once "insight" was gained, for example, in Boring's ambiguous figure of the old-and-young woman, Leeper found it almost impossible to see the other aspect (32). Leeper also found that in material of the type of Street's Gestalt Completion, figures once perceived in a given way were difficult to get rid of, and apparently prevented reorganization. Such percepts represent convergences of high order, so that their destruction is improbable. In order to see any other figure, the integration would have to be rent asunder and dedifferentiation occur, often through many concentric levels. Thus, the earliest anchorages and "associational" integrations precondition later perceptual development and tend to be permanent or to have permanent

effects on further perception and behavior.

It should be pleaded here that there is a good deal of evidence from Freud's studies of cathexis that early trends in this direction are never outgrown. Childhood loves, whether of mothers or of doughnuts, are never wholly lost. In fact, psychoanalysis and other therapies are here presumed to be necessary in part because of early accentuation of certain satisfying aspects of the perceptual field which the organism cannot later leave or submerge effectively under new cathexes. The attempt is made elsewhere (40) to show that this principle, called canalization, is supported by considerable evidence, and to suggest that while such canalizations may be overlaid or disguised, the moment that the overlay is removed, they rebound and reappear in full force.

(16) *Those aspects of a percept which are only prominently "associated" with "satisfaction" tend toward the role of figure but are displaced when this connection ceases.* This hypothesis refers to the evident daily fact that much which tends to attract our notice, and which plays a rather large dynamic part in our feeling and acting, is nevertheless pushed aside when it is no longer related to the satisfaction of our wants. The dinner bell rung repeatedly as a practical joke becomes sheer clanging nuisance. Signals of various sorts which key us up and give us joy hold this role as long as the anticipated satisfaction is actually forthcoming, or as long as we maintain an expectancy (having no reason to believe otherwise); but when the relationship is broken, the role is quickly lost. This may be what is denoted by the extinction of the conditioned response: what was once perceived as "to-be-followed-by" satisfaction, now has lost those *sign-gestalt* characteristics; what once held the center of the stage retreats into relative obscurity.

The difference between these last two points (hypotheses 15 and 16) dealing with "canalization" and "conditioning," respectively, lies in the fact that those anchorages which are themselves directly "satisfying," within which further stable organization has been achieved, cannot be readily discarded; but those which owe their connection with satisfaction only to the temporary advantageous position of "signal" or subgoal status lose it when the connection is broken. In Schafer's experiment, the contours which achieved dominance through reward lost this dominance when reward was discontinued, since such contours were of differential importance in achieving a stable integration only insofar as they were seen as leading to reward. Lambert, Solomon, and Watson (30) found that the size of a poker chip was consistently overestimated (a phenomenon for which no clear-cut explanation exists at present) while that chip served as a subgoal in the path to obtaining a reward. When the reinforcement ceased, the relative overestimation of the poker chip also ceased.

It would accord with these assumptions (13–16) if all learning should prove reducible to a matter of perceptual restructuring. There is a widespread tendency at present to emphasize the flexibility or malleability of perception and to point out that changes in behavior follow directly from changes in the way in which a situation is viewed, so that it is an easy step from this point to the proposition that learning follows from perceptual reorganization. Education and therapy, as many alert observers have suggested, consist of fresh perception.

(17) *Perceptual restructuring can occur at any level of awareness, so that learning while asleep, or learning by invertebrates, etc., need offer no exceptions.* It is suggested that awareness

may be regarded as a question of degree, and, therefore, that while the most explicit, sharply differentiated and richly structured percepts occur at a level upon which introspection can report, there may well be—and probably are—in adults, in children, and in animals, less well-defined perceptual structures which, as a consequence of poor differentiation, defy the precise analysis necessary for introspective report and conscious manipulation. Indeed, one might follow the older suggestions of Leibnitz in carrying awareness down one step at a time until we conceive very vaguely a most primitive and blurred type of experience. Functionally we might go down *below* that and say that even at a level which we cannot regard as conscious at all, processes of structuring and organization occur *as if* they had that quality we know as awareness. Moreover, there is considerable evidence of the existence of *sets* (perhaps directly analogous to what have here been dealt with as perceptual matrices), or "unconscious determining tendencies," of considerable complexity of differentiation (10, 29), whose effects (while obtained without conscious participation) are so similar to those of conscious discrimination and inspection as to suggest the existence of a "preperceptual observer" in much the same fashion as that evidenced by many of the "perceptual defense" studies (35, 36). We see what is coming, so we manage not to see it. The organizational structure of such percepts, and of others suggesting "unconscious perception" (37), is not as yet separable from that of processes in which awareness is evidently present but the processes involved are unmistakably perceptual in nature.

(18) *The more complex phenomena of conflict, ego-defense, and psychoanalytic dynamics generally may be viewed as anchorage and differentiation phenomena in which two or more aspects of a perceptual field can make a sharp bid for the role of figure.* Here it is suggested that instead of being completely unconscious of our own struggling and conflicting tendencies, there are phenomena essentially like those of perceptual conflict within us at all levels of awareness. Awareness of ourselves, of our own inadequacies, of the different directions in which we might like to move; awareness of the modes of escape and of reconciliation of wishes—are all there according to the present thesis, in varying degrees of clarity or obscurity, dissociation or integration.

There is more than one way in which, from this viewpoint, our behavior may be determined by material of which we are unaware, and it would prove instructive to examine the behavioral material presently considered homogeneously as "repression," to determine whether or not it can be fitted into such diverse categories. To select a few possibilities: those aspects of the field which are relegated to ground status still affect the organization but are not noticeable as forms (23). An individual might react to several different objects or situations in the same fashion, not because of an unconscious perception of their similarities—as expressed in the concept of substitution—but because of an inadequate differentiation between them, a lack of perception of their differences (31). Repression might in some cases be analogous to the process by which Schafer's subjects rejected perceptual possibilities which led to frustration, or to that by which Chapman's subjects were unable to report the attributes not included in the *Aufgabe* (10). In other cases perceptual repression might be analogous to an inability to allocate phenomenal causal responsibility to any portion of a relatively amorphous, insufficiently differentiated field; etc.

All that psychoanalysis and the other depth psychologies have taught us may easily be conceived in terms of the learning process, governed by needs and their cognitive expression, exactly as in the simpler experimental examples, except that continuous conflict and inadequate differentiation contribute to the low degree of awareness which some components enjoy, and except that perception of the self in all its rich and complex manifestations must receive cardinal emphasis. But perception of the self could still exhibit the characteristics of perceptual development, figure-ground dynamics, canalization, and the rest of the principles suggested.

SUMMARY

Our hypotheses have dealt with the interaction of exteroceptive, interoceptive, and proprioceptive components in the determination of perception, and with the likelihood that both growth and learning modify the form of this interaction.

It has long been emphasized and, under the stimulus of feed-back theory, it is currently being re-emphasized (63), that as we react to our percepts, our bodies feed in impressions proprioceptively just as our distance receptors feed in, at the same time, reports on the changing situation. What has here been suggested is that it is unjustified to consider one of these components alone as the basis for perception by the normal, functioning organism in its intercourse with its environment, and that the visceral-affective components may also be vitally concerned in perception; that the study of perception includes the affective and conative phenomena and their interrelations with the cognitive in such a way that the whole, living individual participates in every perceptual act; and lastly, a possible relationship has been suggested with the learning process and with those personality dynamics which the psychoanalysts and other clinicians have stressed.

These hypotheses are, of course, only programmatic and worth but little till the hard work of thinking them out, testing them, and modifying them has been done. It is hoped that personality research-methods may in time justify the conception that the perceptual dynamics to which these methods point are ultimately the same perceptual dynamics which in the laboratory can be investigated in terms of figure-ground differentiation and other parameters. There is only one psychology of perception, and it should be equally good in the laboratory and in social reality if it is solidly constructed.

REFERENCES

1. ABEL, T. M. Unsynthetic modes of thinking among adults: A discussion of Piaget's concepts. *Amer. J. Psychol.*, 1932, **44**, 123–132.
2. ASCH, S. E., AND WITKIN, H. A. Studies in space orientation: I. Perception of the upright with displaced visual fields. *J. exp. Psychol.*, 1948, **3**, 325–337.
3. ASHBY, W. R. Dynamics of the cerebral cortex: The behavioural properties of systems in equilibrium. *Amer. J. Psychol.*, 1946, **59**, 682–686.
4. BARTLETT, F. C. *Remembering*. New York: Macmillan, 1932.
5. BICHOWSKY, F. R. The mechanism of consciousness: Pre-sensation. *Amer. J. Psychol.*, 1925, **36**, 558–596.
6. BRUNER, J. S., AND POSTMAN, L. Perception, cognition, and behavior. *J. Personality*, 1949, **18**, 14–31.
7. ———. On the perception of incongruity: A paradigm. *J. Personality*, 1949, **18**, 206–223.
8. BRUNSWIK, E. Psychology as a science of objective relations. *Phil. Sci.*, 1937, **4**, 227–260.
9. CATTELL, R. B. The subjective character of cognition and the pre-sensational development of perception. *Brit. J. Psychol. Monogr. Suppl.*, 1930, No. XIV.
10. CHAPMAN, D. W. Relative effects of determinate and indeterminate *Aufgaben*. *Amer. J. Psychol.*, 1932, **44**, 163–174.
11. DJANG, S. The role of past experience in the visual apprehension of masked

forms. *J. exp. Psychol.*, 1937, **20**, 29–59.

12. DUNCKER, K. The influence of past experience upon perceptual properties. *Amer. J. Psychol.*, 1939, **52**, 255–265.
13. DUSSER DE BARENNE, J. G., GAROL, H. W., AND MCCULLOCH, W. S. The "motor" cortex of the chimpanzee. *J. Neurophysiol.*, 1941, **4**, 287–323.
14. ELLSON, D. G. Hallucinations produced by sensory conditioning. *J. exp. Psychol.*, 1941, **28**, 1–20.
15. FEHRER, E. V. An investigation of the learning of visually perceived forms. *Amer. J. Psychol.*, 1935, **47**, 187–221.
16. HANAWALT, N. G. The effect of practice upon the perception of simple designs masked by more complex designs. *J. exp. Psychol.*, 1942, **31**, 134–148.
17. HEBB, D. O. *The organization of behavior.* New York: Wiley, 1949.
18. HELSON, H. Adaptation-level as a basis for a quantitative theory of frames of reference. PSYCHOL. REV., 1948, **55**, 297–313.
19. HENLE, MARY. An experimental investigation of past experience as a determinant of visual form perception. *J. exp. Psychol.*, 1942, **30**, 1–22.
20. HOCHBERG, J. E. Brightness as a function of perceived spatial form. Paper given at EPA meetings, March, 1951.
21. HUMPHREY, G. *The nature of learning in its relation to the living system.* New York: Harcourt Brace, 1933.
22. KENNARD, MARGARET A. Autonomic interrelations with the somatic nervous system. *Psychosom. Med.*, 1947, **9**, 29–36.
23. KOFFKA, K. *Principles of gestalt psychology.* New York: Harcourt Brace, 1935.
24. KÖHLER, W. Psychological remarks on some questions of anthropology. *Amer. J. Psychol.*, 1937, **50**, 271–288.
25. ——. *Dynamics in psychology.* New York: Liveright, 1940.
26. KRECHEVSKY, I. An experimental investigation of the principle of proximity in the visual perception of the rat. *J. exp. Psychol.*, 1938, **22**, 497–523.
27. KREEZER, G. L. The derivation of the transfer functions of homeostatic systems from experimental response curves. *J. Psychol.*, 1949, **28**, 487–493.
28. KROLIK, W. Über Erfahrungwirkungen beim Bewegungssehen. *Psychol. Forsch.*, 1934, **20**, 47–101.
29. KÜLPE, O. Versuche über Abstraktion. *Ber. ü. d. 1. Kongr. f. exper. Psychol.*, 1904, 56–68.
30. LAMBERT, W. W., SOLOMON, R. L., AND WATSON, P. D. Reinforcement and extinction as factors in size estimation. *J. exp. Psychol.*, 1949, **39**, 637–641.
31. LASHLEY, K. S., AND WADE, MARJORIE. The Pavlovian theory of generalization. PSYCHOL. REV., 1946, **53**, 72–87.
32. LEEPER, R. W. A study of a neglected portion of the field of learning—the development of sensory organization. *J. genet. Psychol.*, 1935, **46**, 41–75.
33. LEVINE, R., CHEIN, I., AND MURPHY, G. The relation of the intensity of a need to the amount of perceptual distortion: A preliminary report. *J. Psychol.*, 1942, **13**, 283–293.
34. MANN, C. W., BERTHELOT-BERRY, N. H., AND DAUTERIVE, H. J., JR. The perception of the vertical: I. Visual and non-labyrinthine cues. *J. exp. Psychol.*, 1949, **39**, 538–547.
35. MCCLEARY, R. A., AND LAZARUS, R. S. Autonomic discrimination without awareness: An interim report. *J. Personality*, 1949, **18**, 171–179.
36. MCGINNIES, E. Emotionality and perceptual defense. PSYCHOL. REV., 1949, **56**, 244–251.
37. MILLER, J. G. Discrimination without awareness. *Amer. J. Psychol.*, 1939, **52**, 562–578.
38. MOORE, J. H. The role of determining tendencies in learning. *Amer. J. Psychol.*, 1936, **48**, 559–571.
39. MOTOKOWA, K. Field of retinal induction and optical illusion. *J. Neurophysiol.*, 1950, **13**, 413–426.
40. MURPHY, G. *Personality.* New York: Harper, 1947.
41. NISSEN, H. W., MACHOVER, S., AND KINDER, ELAINE F. A study of performance tests given to a group of native African Negro children. *Brit. J. Psychol.*, 1935, **25**, 308–355.
42. NOBLE, C. E. The perception of the vertical: III. The visual vertical as a function of centrifugal and gravitational forces. *J. exp. Psychol.*, 1949, **39**, 839–850.
43. ORBISON, W. D. Shape as a function of the vector field. *Amer. J. Psychol.*, 1939, **52**, 31–45.
44. PATERSON, A., AND ZANGWILL, O. L. Disorders of visual space perception associated with lesions of the right cerebral hemisphere. *Brain*, 1944, **67**, 331–358.
45. POSTMAN, L., AND BRUNER, J. S. Unpublished manuscript.

46. ———, AND MCGINNIES, E. Personal values as selective factors in perception. *J. abnorm. soc. Psychol.*, 1948, **43**, 142–154.
47. PROSHANSKY, H., AND MURPHY, G. The effects of reward and punishment on perception. *J. Psychol.*, 1942, **13**, 295–305.
48. RIESEN, A. H. The development of visual perception in man and chimpanzee. *Science*, 1947, **106**, 107–108.
49. ROCK, I., AND FLECK, F. S. A re-examination of the effect of monetary reward and punishment on figure-ground perception. *J. exp. Psychol.*, 1950, **40**, 766–776.
50. ROSENSTOCK, I. M. Perceptual aspects of repression. *Amer. Psychologist*, 1950, **5**, 306–307. (Abstract.)
51. RUBIN, E. *Visuell Wahrgenommene Figuren: Studien in Psychologischer Analyse*. Copenhagen: Glydendal, 1921.
52. RYAN, T. A. Interrelations of the sensory systems in perception. *Psychol. Bull.*, 1940, **37**, 659–698.
53. SCHAFER, R., AND MURPHY, G. The role of autism in a visual figure-ground relationship. *J. exp. Psychol.*, 1943, **32**, 335–343.
54. SHERIF, M. *The psychology of social norms*. New York: Harper, 1936.
55. TOLMAN, E. C. Gestalt and sign-gestalt. PSYCHOL. REV., 1933, **40**, 391–411.
56. VERNON, M. D. *Visual perception*. New York: Macmillan, 1937.
57. VOGEL, P. Über optokinetische Reaktionsbewegungen und Scheinbewegungen. *Arch. ges. Physiol.*, 1931, **228**, 632–644.
58. WALLACH, H. Brightness constancy and the nature of achromatic colors. *J. exp. Psychol.*, 1948, **38**, 310–324.
59. WALLACH, H., AND O'CONNELL, D. N. Perception of tri-dimensional form. *Amer. Psychologist*, 1950, **5**, 487. (Abstract.)
60. WERNER, H. *Comparative psychology of mental development*. Chicago: Follett, 1948.
61. ——, AND WAPNER, S. Sensory-tonic field theory of perception. *J. Personality*, 1949, **18**, 88–107.
62. WEVER, E. G. Figure and ground in the visual perception of form. *Amer. J. Psychol.*, 1927, **38**, 194–226.
63. WIENER, N. *Cybernetics*. New York: Wiley, 1948.
64. WITKIN, H. A. The nature and importance of individual differences in perception. *J. Personality*, 1949, **18**, 145–170.
65. WOOD, H. C. The "haunted swing" illusion. PSYCHOL. REV., 1895, **2**, 277–278.
66. WOODWORTH, R. S. Reënforcement of perception. *Amer. J. Psychol.*, 1947, **60**, 119–124.

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TWO-FACTOR LEARNING THEORY: SUMMARY AND COMMENT *

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I

Whenever one abandons one "faith" and embraces another, there is usually a compulsion to apologize, or at least explain. The explanation, if not apology, in my own case for having deserted the ranks of those who subscribe to a monistic conception of learning and gone over to a form of dualism goes somewhat as follows.

The law of effect, as developed by Thorndike and modified by Hull into a strictly monistic theory of reinforcement, has sometimes been charged with being teleological. In its most primitive form this charge is not hard to refute. In keeping with phraseology laid down by Thorndike, the law has commonly been stated as follows: If a stimulus situation produces a given response and if that response is shortly followed by reward, the response will be strengthened. Critics have rightly lampooned this careless wording of the law, noting that one cannot speak of a response being strengthened by events that *follow* it—that is, not without accepting teleology. The solution, however, is obvious: All we have to do is to note that it is not a particular "response" that is affected by an ensuing reward; rather is it the *responding organism* which is changed, changed neurologically in such a way that in the future its behavior, under similar circumstances, will be altered.

But the spectre of teleology was soon to return in a new guise. In their 1940 book, Hilgard and Marquis (3) called attention to a number of experiments, particularly one carried out by Brogden, Lipman, and Culler (1), in which it appeared that animal subjects were capable of being rewarded by the mere *avoidance* of noxious stimulation. The question which arose here was an awkward one: How could an animal's behavior be influenced, not simply by subsequent events, but by "subsequent events" that do not occur? The procedure used in these experiments was one that Hilgard and Marquis termed "instrumental conditioning." But the use of the term "conditioning" posed a paradox: Conditioning has been assumed to depend upon the association, or paired presentation, of so-called conditioned and unconditioned stimuli; and yet in the studies cited it was found that better "conditioning" is obtained if the CS is followed by the UnCS only when a CR fails to occur, instead of invariably. Here it appears that reward, as well as conditioning, is in some way operating. But to speak of an animal's being rewarded by the non-occurrence of something left much to be desired by way of explication.

The resolution of this paradox was, in one way, relatively straightforward. If a signal has been paired a few times with a noxious event, such as an electric shock, it will, as everyone knows, acquire the capacity to elicit *fear*. And Lamoreaux and I were able to show in a series of studies (8, 9, 10) that a response which is instrumental in averting an impending noxious event gets re-

* This paper and the five which follow were prepared for a symposium which took place at the 1950 meetings of the American Psychological Association at Pennsylvania State College under the auspices of the Division of General Psychology.

inforced, not by the non-occurrence of that event, but by reduction in the intensity of the fear which foreshadows it.

This finding led to a number of other studies [1] which have confirmed the hypothesis that living organisms can be rewarded, relieved, reinforced quite as effectively by a reduction in the intensity of fear or any other form of secondary motivation as by the lessening of a primary drive, such as pain or hunger.

These results seemed, at first, to put reward theory on an even sounder basis as an all-inclusive explanation of learning, since they demonstrated that drive reduction is effective in reinforcing behavior regardless of whether we are dealing with a primary drive or a secondary drive. But on second thought, one began to wonder precisely how it is that acquired, or secondary, drives themselves come into existence. Are they, too, dependent upon reward? Common sense would suggest that it is the temporal coincidence of a signal and pain that makes the signal later elicit fear. But pain usually abates eventually—in the laboratory relatively quickly—and it was therefore possible to hold that, even here, it was drive reduction rather than sheer contiguity that somehow provided the reinforcing state of affairs.

This reasoning seemed to lead to a testable deduction: If the reinforcement for fear learning occurs when the noxious stimulus which is forecast by a signal terminates, and if that signal lasts only until the *onset* of the noxious stimulus, then there should be poorer fear learning when there is a long interval between the onset and end of the painful stimulus than when this interval is of short duration. In other words, under the stated conditions, a signal which is followed by a *brief* painful stimulus should acquire the capacity to elicit a *stronger* fear response than should a signal which is followed by a painful stimulus of prolonged duration. However improbable this inference may sound, it follows logically from the stated premises, for we know from other studies that in conditioning, other things equal, the briefer the interval between a stimulus and the reinforcing state of affairs, the greater the learning.

No one, I believe, has put this particular deduction to an experimental test; and, indeed, from the standpoint of common sense as well as indirect scientific evidence, it would be rather amazing if the deduction were confirmed. However, about four years ago, John Suter and I, using the same general logic, designed and carried out a related experiment which gave completely negative results, *i.e.*, results which were negative as far as the reward theory of fear-learning is concerned. The results pointed to the conclusion that fear learning occurs according to a strict contiguity principle.

These results have already been briefly reported and will be published in detail shortly, so I shall not describe them further here. I do not say that they are "crucial," but they served to give my confidence in monistic reward theory a severe jolt and prompted me to make a rather searching examination of the possibility that there are two forms of learning: (1) sign learning, or conditioning, on the one hand, and (2) solution learning, or problem solving, on the other. The results of that examination were published in 1947 (5) and a study entitled "Further evidence for a two-factor theory of learning" is now in press (6). On the basis of these inquiries I have become a confirmed dualist.

II

Here I shall give only a brief resumé of the two-factor conception of learn-

[1] See Miller's recent review (4).

ing which I now believe most defensible. This I shall do under four headings:

(1) *Definitions.* One of the chief obstacles to the emergence of a clear-cut, two-process conception of learning has been the non-precise way in which conditioning and problem-solving have often been defined. Certainly a conception of problem-solving, or trial and error, which restricted this form of learning to behavior motivated exclusively by primary drives, was inadequate and incomplete; what we needed to recognize is that reward learning can and does occur with equal facility when the underlying problem or drive is acquired rather than biologically given. But the principal problem arose in connection with a tendency to over-extend the term conditioning and make it include many responses which qualify very acceptably as problem solving when this latter process is properly conceived. In order to make a two-factor theory work, "conditioning," it now appears, must be restricted to the process whereby emotions, meanings, attitudes, appetites, and cognitions are acquired, with "solution learning" applying to all cases of overt instrumental habit formation.

(2) *Anatomy.* Solution learning seems to be largely dependent upon the central nervous system and to involve responses which are mediated by the skeletal muscles, whereas sign learning importantly involves the autonomic nervous system and is manifested by viscero-vascular reactions. The first class of responses are, in the main, voluntary, and the latter involuntary.

(3) *Description and Measurement.* Sign learning is often characterized, quite correctly, as involving stimulus substitution or associative shifting (Thorndike), whereas solution learning is often described as involving response substitution. The one may be properly said to lead to responses that are anticipatory and the other to responses that are consummatory (Sherrington). In terms of the laboratory methods which are best adapted to the study of these two forms of learning, sign learning is usually represented as an incremental function (incidence of response) and solution learning as a decremental function (reduction in time or errors).

(4) *Implications and Supporting Evidence.* In the 1947 paper already cited, it was shown that two-factor theory satisfactorily resolves a number of contradictions and paradoxes of long standing. In the interim I have found it useful in the analysis of language learning, perceptual phenomena, identification, and neurotic processes. Collateral evidence, which I had previously overlooked or which is being obtained through new experimentation, continues to accumulate and will be presented in a publication which will appear shortly (6).

III

In the time that remains I should like to refer briefly to a recent paper by W. Horsley Gantt (2), entitled "Psychosexuality in animals," by way of illustrating the continuing emergence of evidence for a two-factor conception of learning, sometimes from unexpected quarters. The main object of Dr. Gantt's paper is to show, on the basis of a wide range of evidence, that sexual excitement has somewhat the same effect as does sedation or reassurance: it brings about a reduction of fear and anxiety. Perhaps this is a substantiation of the familiar assumption that the sympathetic and the parasympathetic divisions of the autonomic nervous system act antagonistically and that anything that will powerfully excite the one tends to depress activity mediated by the other. Here, perhaps, is an example parallel to what is known clinically as the "anxious eater," the person who eats, not just to allay hunger, but who

uses oral and gustatory stimulation as a means of producing parasympathetic excitement and quelling, at least for the moment, affective states mediated by the sympathetic division.

This jockeying of one division of the a. n. s. against the other and its bearing upon a two-factor conception of learning has been considered elsewhere (5). Here we are concerned with a more or less incidental observation in Gantt's paper, but one that bears centrally on two-factor theory as formulated above. Gantt says:

"Finally, the comparative physiology of the cardiac, secretory and motor conditional reflexes reveals what may be a basic constitutional mechanism for nervous dysfunction on the higher level of behavior. Our studies show that the cardiac component is formed more quickly than the motor component of the conditional reflex to pain and that the cardiac component persists much longer.

"The cardiac component is both more sensitive in appearing first and paradoxically more stable in persisting longer. Without reinforcement, it has endured as long as two years after the motor component has dropped out. If the cardiac acceleration represents the inner emotional aspect, and the motor conditional reflex the specific external component, we have the picture of an organism externally in adaptation to the environment but internally excited and reacting to the trace of a conditional stimulus which long since ceased to have its former significance. The animal has achieved external adaptation but emotionally it is maladapted. Thus, there is a kind of cleavage between the outer specific muscular and the inner cardiac expression" (2, pp. 49–50).

It is evident that Gantt is not thinking here in terms of two-factor learning theory, but the reported facts are congruent with such a theory. They show, as have other studies, that in an experimental situation involving painful experiences, the fear (cardiac) reaction is acquired first and that the motor responses which then appear are indeed *caused by* the fear and represent efforts on the part of the organism to deal therewith. With the passage of time or the occurrence of extinction, the underlying emotional reaction may gradually weaken to the point where it will no longer elicit overt, problem-solving behavior but may still be detectable as a faint conditioned, *i.e.*, physiological, reaction.

In this context Gantt speaks of an interesting phenomenon which I recall hearing Walter R. Miles refer to orally. Gantt says:

"Observations have been made by Whitehorn in patients and by Hoffmann and me in dogs that are perhaps related . . . viz. the fact that in an individual, at least when a spontaneous movement is made, it is preceded rather than followed by an acceleration of heart rate. The nature of the spontaneous movements, i.e., whether the dog changes from sitting to standing or vice versa or merely shifts his weight, is immaterial—first there is the cardiac acceleration, then the movement, or as Hoffmann has put it, 'first comes the heart rate and then the dog.' From these observations of normal behavior, it appears that some natural excitation in the central nervous system, expressed by an increase in heart rate, not only results in but rapidly expands itself in muscular movements" (2, p. 48).

These two sets of observations by Gantt graphically emphasize the double function of visceral mobilization of the type which is represented by increased heart rate. Gantt's latter remarks in this connection show that visceral mobilization may occur simply by way of *physiological preparation* for action on the part of the skeletal musculature, action that may be motivated by hunger, postural tension, or the like. But before an organism, at least before a "higher" organism, makes a gross, overt response under these conditions there is, presumably, an "intention" to act, con-

sisting of an implicit mediating (symbolic) reaction of some kind; and this, having commonly preceded gross muscular activity (which depletes physical energy and thus reflexly elicits physiological activity), has therefore become a "conditioned stimulus" for physiological activity. Here we appear to be dealing with autonomic-visceral reactions which are proportional to the physiological needs of the organism and involve no element of over-preparation, no excessive internal pressure or tension, no motivation. But when autonomic-visceral reactions occur which exceed such needs, they take on motivational properties, and we call them, or at least the sensations they produce, *emotions*.

We see, therefore, that conditioned reactions, in the strict sense of that term, may serve one or both of two purposes: They may provide a physiological background, or substructure, for energetic, overt behavior; and they may also serve to motivate, or cause, behavior. A conditioned reaction may therefore be anticipatory in a double sense: it may anticipate some external event, such as pain or injury, and in so doing provide motivation for behavior which serves to evert the event; but, at the same time, the conditioned reaction also anticipates the physical needs that will be created by such action—or by action which is created, as in the second type of example given by Gantt, by other needs.

Conditioned responses, as we thus see, are intimately related to physiology, as distinct from problem-solving behavior, which is mediated by the central nervous system and the skeletal musculature. It is perhaps not unreasonable to suppose that such very different types of activities on the part of living organisms should exemplify two distinct learning processes.

BIBLIOGRAPHY

1. BROGDEN, W. J., LIPMAN, E. A., AND CULLER, E. The role of incentive in conditioning and extinction. *Amer. J. Psychol.*, 1938, **51**, 109–117.
2. GANTT, W. H. Psychosexuality in animals. In P. H. Hoch & J. Zubin (Eds.), *Psychosexual development in health and disease*. New York: Grune & Stratton, 1949.
3. HILGARD, E. R., AND MARQUIS, D. G. *Conditioning and learning*. New York: D. Appleton-Century, 1940.
4. MILLER, N. E. Learnable drives and rewards. In S. S. Stevens (Ed.), *Handbook of experimental psychology*. New York: John Wiley & Sons, 1951.
5. MOWRER, O. H. On the dual nature of learning—a reinterpretation of "conditioning" and "problem solving." *Harvard educ. Rev.*, 1947, **17**, 102–148.
6. ——. Further evidence for a two-factor theory of learning. Chap. 10 in *Learning theory and personality dynamics*. New York: Ronald Press, 1950.
7. ——. On the psychology of "talking birds"—a contribution to language and personality theory. Chap. 22 in *Learning theory and personality dynamics*. New York: Ronald Press, 1950.
8. ——, AND LAMOREAUX, R. R. Avoidance conditioning and signal duration—a study of secondary motivation and reward. *Psychol. Monogr.*, 1942, **54**, No. 5.
9. ————. Fear as an intervening variable in avoidance conditioning. *J. comp. Psychol.*, 1946, **39**, 29–50.
10. ————. Further analysis of the concept of "conditioning." PSYCHOL. REV., 1951, **58**, 196–212.
11. ——, AND VIEK, P. An experimental analogue of fear from a sense of helplessness. *J. abnorm. soc. Psychol.*, 1948, **43**, 193–200.

SENSORY INTEGRATION AND COGNITIVE THEORY

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The question of whether a multiplicity of processes must be postulated in order to account satisfactorily for the phenomena of animal learning poses two independent but related problems. The first is a phylogenetic problem for which the facts of comparative anatomy suggest a clear answer. Since the behavior of the different animal species is modified through the operation of different organic systems, the question, from the phylogenetic viewpoint, is no longer *whether* a multiplicity of processes exists but *what* are the characteristics of the diverse processes that make learning possible at the various levels of animal life. The second problem is intra-specific rather than inter-specific. It is concerned with the nature of the processes which underlie learning in any given animal form. This paper, then, is divided into two sections. In the first we shall deal briefly with some of the implications of comparative psychology for learning theory, while in the second we shall be concerned with the intra-specific problem. We have chosen to consider cognitive theory in our discussion because we feel that it provides an excellent vehicle for the analysis of both aspects of the multiple-process problem.

There can be little doubt that the most influential criticism of a pure reinforcement theory of learning has been provided by the cognitive theorists under the leadership of Professor Tolman (15). The effectiveness of this criticism is revealed both by the amount of experimental work which reinforcement theorists have devoted to attacking the weaker aspects of Tolman's position and by the silence they have maintained concerning some of the most crucial items of evidence adduced by the cognitive theorists. However, although we are indebted to Professor Tolman for repeatedly calling the attention of psychologists to phenomena of learning which stimulus-response psychologists have tended to overlook, we believe that his systematic point of view shares a vital weakness with reinforcement theory. Both positions are anti-comparative, functionally if not explicitly, in that they lead to formulations which are so gross as to be applicable to all animal forms—not only to rat, the favorite subject for investigation, but to paramecium and to man. True, Professor Tolman's occasional reference to "capacity laws" may be regarded as a bow in the direction of phylogenesis, yet it is difficult to see why the educated paramecium of Day and Bentley (4) could not be characterized, in Tolman's terms, as a conscious, purposeful creature which develops cognitive maps (however narrow) and acquires sign-gestalt-expectations. In Professor Hull's terms, of course, changes in the behavior of the trained paramecium might be thought of only as another instance of the strengthening of stimulus-response connections under the influence of need-reduction.

The adoption by both theorists of what has become known as the molar

viewpoint leads inevitably to the blurring of phylogenetic distinctions. It is sometimes useful in the preliminary stages of research to think of behavior in functional terms which make no immediate reference to underlying bodily processes, but it is inexcusable to cling doggedly to gross formulations which are based upon the end results of behavior and which are permanently divorced from underlying mechanisms. The processes of modification in paramecium and rat are not qualitatively identical because the two animal forms are anatomically quite dissimilar. Any theoretical approach which glibly obscures this difference should be recognized as the most dangerous variety of subjectivism. The so-called molar view is sometimes justified in the name of parsimony, but this clearly is to misunderstand the meaning of the concept. In the case of the paramecium—to return to our rather extreme example—the superficiality of both molar formulations is demonstrated in an experiment suggested by Buytendijk (3). Preliminary immersion of a naive control animal in a solvent renders it just as capable of escaping from the capillary tube as an experimental animal patiently practiced after the manner of Day and Bentley. This finding makes it clear that improvement in the trained animal may be the product of increased flexibility resulting from activity-produced chemical changes in protoplasmic constituents, and may have no relation to the specific stimulating conditions encountered in training, or to the nature of the ends attained by the responses which terminate each training trial. Theoretical frameworks of the molar sort not only provide no pegs on which to hang such findings, but tend to discourage the fundamental research which produces them. Here is parsimony by exclusion—by exorcism.

This common failing of cognitive and reinforcement theory must not be obscured by the fact that the two formulations are couched in very different language. Professor Tolman likes to talk in phenomenal terms frankly borrowed from the introspectionists, while Professor Hull prefers a dialect of the Watsonian language. Certainly Professor Tolman's formulations are dangerously anthropomorphic, but in a fundamental sense so also are those of Professor Hull. Anthropomorphism is dangerous not because it humanizes animals, but because it encourages us to be content with conceptions which are nothing more than loose analogies and which therefore obscure real differences among phylogenetic levels. Hullian theory falls into the same error. Only the *kind of analogy* upon which it is based is different.

A psychology without mind may be conceivable, but a psychology without mind *or* body is not. If we take the facts of evolution seriously, then the answer to the first question before us is clear. There is not one learning process, but many. We should not be at all surprised to find qualitatively distinct processes at every major level of anatomical organization. Certainly we may expect to find general processes which are essentially similar over a wide range of organisms in the phylogenetic series, but we cannot be content until we have found differences in functional capacity consonant with the wide differences in structural organization which confront us.

The history of learning theory shows an ever increasing gap between the phenomena which are presumably under consideration and the theoretical systems that have been constructed to deal with them. It is only living organisms that learn, and any systematic discussion of learning must, if it is to remain relevant, refer to the characteristics of these organisms in all their similarity

and diversity. Behavior which characterizes the rat may be quite atypical of the earthworm, and even the briefest glance at man demonstrates his difference from the stereotyped ant or the lowly amoeba. Operational ease cannot be substituted for the hard digging of science, and the ubiquitous white rat is an inadequate model upon which to base our conceptions of the psychological natures of all other organisms. In the concentrated and often heated activity of system making, the simple truth that organisms exist at different phylogenetic levels has been lost, and with it the essential problem for learning theory—the search for the mechanisms which make behavioral modification possible.

When we turn from the broad phylogenetic problem to an inquiry into the nature of modification in any given animal form, such as the rat, similar considerations are relevant. Here we no longer have so strong a reason to anticipate the discovery of a multiplicity of qualitatively distinct processes rather than a single basic process, but once again it may be well to proceed cautiously. Professor Tolman has suggested that "our familiar theoretical disputes about learning may *perhaps* . . . be resolved, if we can agree that there are really a number of different kinds of learning. For then it may turn out that the theory and laws appropriate to one kind may well be different from those appropriate to other kinds. Each of the learning theories now current may, in short, still have validity for some one or more varieties of learning, if not for all" (16, p. 144). He then goes on to recognize the validity of reinforcement theory with respect to the acquisition of "positive cathexes" (although he hastens to add that there is no good evidence for the view) and, in the matter of "motor patterns," allows Guthrie to win by default. In our opinion this genial eclecticism, although in many respects admirable, misses the important point. While it is true that each of the major theories has tended to focus on limited aspects of learning, we can by no means be sure that any given theory deals adequately even with those phenomena with which it has been most directly concerned. The *data* obtained in experiments conducted by adherents of the various schools must be given respectful consideration, but it does not follow that the *explanations* advanced to account for these data can be accorded equivalent status and set side-by-side to make a comprehensive theory. Lumping together the reports of the seven blind men does not produce an adequate description of the elephant.

In a recent paper we have defended the position that at least two qualitatively distinct learning processes must be postulated in order to deal adequately with available data on the learning of lower mammals (1). On the one hand there is the organization and reorganization of motor patterns which occurs within the confines of a given environment. The animal selects, refines, and fixates those responses which are appropriate to the relationship which exists between internal and external conditions. This is the problem with which reinforcement theorists have been almost exclusively concerned, and the evidence to some extent justifies their theoretical emphasis on afferent-efferent integrations and the role of biological satisfactions. But we are still very much in the dark with respect to the nature of the process or processes which make selective learning possible. Hull's fourth postulate (6) does not begin to deal with this problem, but in fact avoids it. Not many years ago learning theorists were apt to pose a fundamental question relating to the empirical law of effect. In what way,

they asked, do the satisfying consequences of a response work back upon the organism to increase the likelihood that the response will appear on subsequent contacts with the situation? Unfortunately, in this heyday of modern positivism, such questions have become exceedingly unpopular.

The data of conditioning experiments, on the other hand, suggest the operation of a second process which seems much easier to understand. Following Maier and Schneirla (8) we have chosen to refer to it as a process of sensory integration. When two afferent centers are contiguously activated, a functional relation is established between them such that the subsequent innervation of one will arouse the other. Here we postulate a purely afferent process of modification which may operate not only in the absence of concurrent motor activity but in the absence of need-reduction as well. This process is revealed most directly in the development of stimulus-equivalence which occurs in conditioning—when two stimuli are presented contiguously, the first acquires some of the functional properties of the second. It also helps us to understand many of the phenomena which occur in other learning situations, especially those phenomena which, as the cognitive theorists have noted, cannot be dealt with adequately by Hullian theory. It should be recognized, of course, that the concept of sensory integration has long been implicit in the cognitive formulations. Some years ago Professor Spence (12) made the point clearly when he labeled Professor Tolman's position an "S-S Contiguity" theory.

In criticism of stimulus-response-reinforcement theory, the cognitive studies reiterate one essential point, namely, that animals may respond appropriately under changed motivational or environmental conditions even though the responses elicited are not identical with those previously practiced. This means that in the course of learning integrations are developed which cannot easily be understood as the attachment of responses to stimuli. However, cognitive theorists have not been concerned with the mechanisms underlying such behavior, but have contented themselves with phenomenal analogies to a presumptively understood human consciousness.

The important studies of Blodgett (2) and Tolman and Honzik (18), which clearly demonstrate that the rat learns a great deal about a maze under conditions which preclude the reinforcement of specific response patterns, have provided the strongest support for the cognitive position. That the data of these experiments pose an extremely difficult problem for stimulus-response-reinforcement theory is attested by Hull's failure even to mention them in his *Principles of Behavior*, and by the scant attention which they receive from S-R theorists in papers on the more recent, though directly related, researches with the T-maze. In our opinion the principle of sensory integration makes it possible to understand these results.

It might be profitable to ask once more the familiar question of how a rat learns to take the true-path turn, rather than enter the cul, at a choice-point in a maze. The traditional answer of response-reinforcement theory is about as follows: The complex cues (interoceptive as well as exteroceptive) which are present at the choice-point become individually, and as a group, connected to the response of turning in the correct direction on the basis of ensuing reinforcement, primary or secondary. But the concept of sensory integration suggests an alternative interpretation. From this point of view, not one, but two stimulus configurations may function at the choice-point, at least in the early stages of learning—one arising from the

mouth of the cul and the other from the entrance to the true path. When the animal takes either path a succession of new afferent patterns arise, in the course of movement, with which the entrance-pattern (directly or mediately) becomes functionally equivalent. For example, when the animal enters a cul the entrance-percept is followed sooner or later by a characteristic obstruction. If the blind is both short and homogeneous, the closely contiguous relation between entrance- and end-patterns gives rise to a process of afferent integration and the equivalence is readily established. When motivational conditions are such that avoidance-behavior would be evoked by the cul-end, this behavior will now be evoked by the cul-entrance. In longer and less homogeneous blinds, such as those which involve a number of turns, there is a succession of critical percepts (principally at the elbows of the cul), and end- and entrance-percepts are less immediately related. Under these conditions cul-elimination proceeds in stepwise fashion, backwardly from end to entrance, with the units of blind-alley pathway usually being eliminated as wholes. From our point of view, the less rapid elimination of long blinds is explained, not in terms of a temporal gradient of reinforcement, but in terms of the temporal relation between stimulus patterns arising at cul-entrance and cul-end. The great difficulty which is presented by circular blinds is to be understood from the fact that they provide no characteristic barrier-percept. The principle of sensory integration also leads to the assumption that choice-point learning in the early sections of a complex maze is to be understood, at least in the initial stages of learning, not in terms of the development of true-path preferences, but rather as a process of cul-avoidance, since the afferent consequences of correct turns are less structured and less directly significant than those of cul-entry. The true path units do, of course, eventually acquire more positive properties, but in a more gradual fashion due to their remote relation to the goal-object, and the very rapid learning which occurs at later points in the maze can be interpreted as a joint function of integrations which develop with respect both to cul- and true-path entrances. It might also be suggested that "latent" learning in a complex maze is largely confined to the region of the culs, as an experiment by Reynolds (10) has indicated, and to the goal-box which has a rather special afferent character derived from the removal of the animal from the maze.

In order to deal with serial learning in terms of reinforcement theory, Hull is forced to rely heavily on the principle of secondary reinforcement which bridges the temporal gap between early responses in the series and the terminal "primary" need-reduction. According to this principle, which is regarded as a corollary of the fourth postulate, stimuli which arise in the course of running not only become guides to action, in the sense that they are connected to appropriate responses, but they—the same stimuli—also acquire goal properties which are mediated in a backward direction. In some respects this hypothetical process resembles our own conception of the manner in which true-path units acquire positive properties, but there are important differences. Hull maintains that the development of secondary reinforcement is itself a process of stimulus-response conection, in which the stimuli in question become attached to "fractions" of the response patterns involved in need reduction. Afferent relations are not, in this view, established directly, but must always be response-mediated, and these relationships can only be established when one of the stimuli itself arouses need-reducing responses.

Now it is our position that all of the

data with which Hull deals in terms of the concept of secondary reinforcement can be dealt with equally well if one assumes a process of sensory integration in which relations between afferent processes, or sensory equivalences, are established, not indirectly, but *directly*, in the course of contiguous arousal, and which may occur in the absence of drive-decrement. Furthermore, this assumption makes it possible to account for the classical latent learning data, while Hull's conception does not. From our point of view the latent learning experiment may be understood as a complication of the sensory pre-conditioning experiment. It must be emphasized that not only a contiguity principle, but an *afferent* contiguity principle, is required by this evidence. Neither the version of Professor Guthrie (5) nor the version of Professor Mowrer (9) will do.

In recent years cognitive theory has stimulated a variety of experiments with the T-maze, which are closely related to the latent learning problem, and which point even more clearly to a sensory integration process. For the most part, these experiments may be grouped under three major headings. In all three the rat is trained in the maze, food being situated in one arm and water in the other, and later tested under motivation which corresponds to one or the other of these incentives. In the first type of experiment the animal is satiated for both food and water during training and in the second type of experiment the animal is both hungry and thirsty during training. The results of both types of experiment show a significant tendency for the rats to respond appropriately when singly motivated—hungry or thirsty. Even if reinforcement operates in both experiments—secondary reinforcement may be presumed to operate in the first—the forced-turn procedure employed in training should keep the alternative response-tendencies approximately equal in strength, and the animal should respond randomly. Even if turning tendencies of unequal strength become established—due perhaps to the greater strength of one or the other of the drives or to the greater potency of one or the other of the reinforcements—the animal cannot be expected to respond appropriately under *both* conditions of single motivation in the test trials. The *ad hoc* principle of the selective association of drive-stimuli (7) may account for the results of the second type of experiment, but it will not account for those of the first type in which there are no drive stimuli to be selectively associated. The last resort for the reinforcement theorists is the untestable concept of unobservable anticipatory response (13, 17).

In a third major type of experiment stimulated by cognitive theory, the animal is trained under one drive and tested under the second. Here the animal fails to respond appropriately, and the reinforcement theorists, as might be expected, have made much of this fact. A recent experiment by Walker, Knotter, and DeValois (19) suggests, however, that the forced-turn procedure ordinarily employed may in great measure be responsible for the negative results. Trained under conditions of water deprivation, with water in one goal box and both food and water in the other, their animals did respond appropriately when the drive was shifted to hunger. In another variation of the basic design Strange (14) found clear evidence of latent learning under single drive conditions.

In conclusion, there are two points that we should like to make. First, there is a good deal of other evidence which, in our opinion, can be better understood in terms of the concept of sensory integration than in terms of either cognitive or reinforcement theory. Our purpose here was only to show how the idea of sensory integration which was

developed originally to deal with the results of conditioning studies could be applied to behavior in other standard learning situations. Secondly, we should like to note that the sensory integration postulate is regarded, not as a final solution to our problems, but as a basis for further research. What conditions other than contiguity of stimulation affect the integration process, and in what manner? Do the afferent consequences of motivational states play a special role? It seems that they do, although it is likely that the *onset* and *presence* of these afferents are just as significant as their *decline* and *disappearance* (11). Above all, there is the question of how an understanding of sensory integration will make it possible for us to deal with the problem of selective learning. Traditional stimulus-response theory is based on the assumption that any stimulus may be tied to any response. But suppose we say instead that, not stimuli, but afferent organizations or afferent integrations produce characteristic patterns of response, and that for each pattern of afferent organization there can be one, and only one, pattern of response. It follows directly from this formulation that the process of selective learning can be considered, not as a process of stimulus-response connection, but as *a process of achieving the afferent organization required to produce a given pattern of response.* Here at last we may have a meaningful, process-oriented translation of the dictum of the cognitive theorists: Given knowledge, behavior will take care of itself.

BIBLIOGRAPHY

1. BIRCH, H. G., AND BITTERMAN, M. E. Reinforcement and learning: The process of sensory integration. PSYCHOL. REV., 1949, 56, 292–308.
2. BLODGETT, H. C. The effect of the introduction of reward upon the maze performance of rats. *Univ. Calif. Publ. Psychol.*, 1929, 4, 113–134.
3. BUYTENDIJK, F. J. Acquisition d'habitudes par des êtres unicellulaires. *Arch. Neerl. Physiol.*, 1919, 3, 455–468.
4. DAY, L., AND BENTLEY, M. A note on learning in paramecium. *J. anim. Behav.*, 1911, 1, 67–73.
5. GUTHRIE, E. R. *The psychology of learning.* New York: Harper, 1935.
6. HULL, C. L. *Principles of behavior.* New York: Appleton-Century, 1943.
7. KENDLER, H. H. The influence of simultaneous hunger and thirst drives upon the learning of two opposed spatial responses in the white rat. *J. exp. Psychol.*, 1946, 36, 212–220.
8. MAIER, N. R. F., AND SCHNEIRLA, T. C. Mechanisms in conditioning. PSYCHOL. REV., 1942, 49, 117–134.
9. MOWRER, O. H. On the dual nature of learning—a reinterpretation of 'conditioning' and 'problem-solving.' *Harv. educ. Rev.*, 1947, 17, 102–148.
10. REYNOLDS, B. A repetition of the Blodgett experiment on 'latent learning.' *J. exp. Psychol.*, 1945, 35, 504–516.
11. SHEFFIELD, F. D., WULFF, J. J., AND BACKER, R. Reward value of copulation without sex-drive reduction. *J. comp. physiol. Psychol.*, 1951, 44, 3–8 (cited by Sheffield).
12. SPENCE, K. W. Theoretical interpretations of learning. In F. A. Moss (Ed.), *Comparative psychology.* New York: Prentice-Hall, 1942.
13. ——, BERDMANN, G., AND LIPPITT, R. A. study of simple learning under irrelevant motivational-reward conditions. *J. exp. Psychol.*, 1950, 40, 539–551.
14. STRANGE, J. R. Latent learning under conditions of high motivation. *J. comp. physiol. Psychol.*, 1950, 43, 194–197.
15. TOLMAN, E. C. *Purposive behavior in animals and men.* New York: Appleton-Century, 1932.
16. ——. There is more than one kind of learning. PSYCHOL. REV., 1949, 56, 144–155.
17. ——, AND GLEITMAN, H. Studies in learning and motivation. I. Equal reinforcement in both end-boxes, followed by shock in one end-box. *J. exp. Psychol.*, 1949, 39, 810–819.
18. ——, AND HONZIK, C. H. Introduction and removal of reward, and maze performance in rats. *Univ. Calif. Publ. Psychol.*, 1930, 4, 257–275.
19. WALKER, E. L., KNOTTER, M. C., AND DEVALOIS, D. L. Drive specificity and learning: The acquisition of a spatial response to food under conditions of water deprivation and food satiation. *J. exp. Psychol.*, 1950, 40, 161–168.

THE CONTIGUITY PRINCIPLE IN LEARNING THEORY

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The last twenty years of basic learning theory has reflected a period of the supremacy of the law of effect and the discounting of contiguity. This trend started when conditioned response people began noticing that conditioning was often complicated by selective learning which did not seem to follow the Pavlovian rubric. We can perhaps identify the starting point of this era with the 1928 article by Miller and Konorski (12) on a special type of conditional reflex—later to be named the conditioned or discriminated "operant" (19), the "instrumental" conditioned response (7), and so forth. The development subsequent to Miller and Konorski has been one of rediscovering the law of effect—and relegating contiguity to a minor role either as a primitive learning principle among dualists or as a necessary but insufficient factor among the single-process effect theorists.

Now, I suspect, we are on the threshhold of a rediscovery of contiguity. I think Mowrer's paper announcing his switch to dualism in the 1947 *Harvard Educational Review* (13) is one of the signs of the times. Also the 1949 Birch and Bitterman (1) critique of the effect position—and support of Maier and Schneirla (10) on dualism—I interpret as an undercurrent of dissatisfaction with the recent pre-eminence of effect as the major learning principle. Mowrer has elevated contiguity to an equal status with effect as a sometimes-sufficient condition for learning. Birch and Bitterman have hinted that their sensory integration version of contiguity will handle phenomena which even previous dualists have considered the exclusive property of effect. In each case these authors suggest a trend toward reinstating the significance of contiguity as a sufficient condition for some learning.

At the same time there seems to be another sort of trend underway in favor of contiguity. This second trend I would say is more relevant to contiguity as a *factor* than contiguity as sufficient. It consists of the discovery that contiguity is a powerful source from which to derive learning phenomena without regard to one's position as to whether contiguity is sufficient—or only necessary. A good illustration is seen in Estes' (3) recent article in which he disowns the reinforcement problem and proceeds to deduce a mathematical model of selective acquisition based on a very Guthrian (6) set of assumptions flowing from contiguity alone. Other illustrations are seen in modern interpretations of partial reinforcement, as presented in the Jenkins and Stanley (9) review article, and in the interpretations of extinction and spontaneous recovery in Skinner's (20) very recent paper challenging theories in general and S-R reinforcement theories in particular.

Perhaps my perception of the current situation is biased, but I feel that we are getting further and further in handling the major phenomena of learning with less and less worry about reward as a factor. Perhaps we can carry it all the way and eliminate the law of effect from a general treatment of learning. I do not feel entirely optimistic about this parsimonious outcome but I think it is worth a concerted effort. And at the moment I would place the dualist position as a halfway point in such an

effort. It strikes me as an easy out—with defeatist consequences—for a current situation in which neither contiguity nor effect can make a convincing universal case.

On a less intuitive basis my objections to dualism are easily classified: (1) there appears to be strong evidence that learning—when properly defined—is a single process, and (2) the arguments of the dualists are unconvincing whether one takes a single-principle contiguity approach or whether one plays devil's advocate and takes a single-principle effect approach.

Starting with the first of these—the evidence for a single process—my point is simply that the phenomena exhibited in selective learning are so similar to those of Pavlovian conditioning that it is hard to believe in any differences in physiological process. One should of course be prepared to expect that two different physiological mechanisms will have the same overall outcome—as when temperature regulation is achieved both by perspiration and by vasodilation. But one should not be prepared to expect that two different mechanisms will exhibit point-by-point similarity in their detailed properties. Yet this is the case in a conditioned modification and a selectively acquired one. The ease with which the S-R reinforcement people have been able to appropriate Pavlov's findings attests to the similarity of the details of the two forms of learning. In such matters as acquisition, extinction, spontaneous recovery, generalization, differentiation, effects of partial reinforcement, and so forth, it makes no difference whether we are talking about a Thorndikian or a Pavlovian learning situation. The impressive fact is that we can translate from one discipline to the other by the simple expedient of interchanging appropriately the terms "reward" and "unconditioned stimulus." To me this makes it very improbable on *a priori* grounds that there is not a common process. The notable differences are not in properties but rather in inferred experimental operations in reinforcing with an unconditioned stimulus on the one hand and with a reward on the other. This suggests looking extensively for a hidden similarity in operations or process—it does not readily suggest that there is a different process to go with each experimental operation.

In my second objection to dualists—that their arguments are not convincing—I shall not attempt to cover all dualists. Instead I will mention only present company. I am not impressed by the Birch and Bitterman argument because it hinges primarily on what I consider to be an incorrect analysis of avoidance training. Here I have to play devil's advocate and defend the law of effect. Their main objection to a single-principle effect theory is that they believe the findings of avoidance learning disprove the Hullian hypothesis that cessation of pain is a reward. Their disproof is too easy to be true. The argument runs as follows: in avoidance training *pain* and its termination is invariably correlated with failure to respond; *no pain* is invariably correlated with the correct response. Therefore, they argue, according to Hull failure to respond is rewarded by pain termination whereas performance of the correct response is subjected to extinction through non-reward. Since the facts of avoidance learning are just the opposite, Hull's notion of the reward value of pain termination is said to be disproven.

However, Birch and Bitterman have ignored entirely the factor of conditioned fear as a drive—and reduction of fear as a reward. These factors play a critical role in modern effect interpretations of avoidance learning as formulated by Mowrer (13) and others (14, 16). In this interpretation the

critical fact is that failure to make the correct response becomes very frightening because it invariably produces pain. At the same time performance of the correct response cannot become frightening because it is invariably not correlated with pain. Thus in avoidance training we create an instrumental learning situation—with fear as drive—and in which the only way to convert a frightening situation into a nonfrightening one is by lifting the paw, leaping into the air, or whatever else the experimenter might require. This can be handled by Hull (8) in drive-reduction terms or by Guthrie (4) in stimulus-change terms.

I am more impressed with the Birch and Bitterman argument that sensory pre-conditioning is an exception to the law of effect, although I am also familiar enough with effect dialectics to know that there are several ways out. For one we can postulate an investigatory drive which is satisfied by attending to any and all environmental changes. For another we can note that with strong stimuli the mere cessation of the stimulation is reinforcing to whatever response the animal makes to the stimuli. Thus we may predict that the response to S_1 becomes connected to S_2 and vice versa—and will mediate just about the amount of transfer actually obtained—namely, a *small* amount in most cases. Moreover, the usual smallness of transfer in sensory preconditioning appears to be inconsistent with their sensory-integration interpretation of Pavlovian conditioning. Thus the Birch and Bitterman argument can be said to backfire considerably if the question is asked as to why the phenomenon is always such a weak counterpart of direct conditioning. If all conditioning by contiguity is afferent modification, why did Brogden (2), in the original experiment on this topic, find that some subjects failed to show any transfer, the average transfer was only 21 per cent of direct conditioning, and what little transfer was obtained initially extinguished very rapidly?

The Mowrer and Suter (15) experiment will also be attacked by single-process effect theorists. For example, does a signal that precedes shock, accompanies shock, and terminates with shock become a cue for fear or a mixed cue for fear and the relaxation of fear that accompanies the termination of shock? Or, since the animal is free to move about on a grill, what evidence is there that all animals do not momentarily stop shock immediately by partial hops from the grill? Or, to be technical, what control is offered for an Adrian-type "on effect"—with an immediate and sharp pain reduction through sensory adaptation—as suggested by Miller and Dollard (11) when a cue is correlated with onset of pain?

Personally I think the Mowrer and Suter experiment *is* embarrassing for the effect position but not a crucial exception. On the other hand, it goes almost without saying that the experiment would not convince a contiguity theorist that one is forced into dualism; rather it is merely further support that contiguity is sufficient. Similarly Mowrer's (13) "collateral support" for dualism, while it helps make the dualistic position reasonable, does not at all show one is forced into dualism. The hypothesis that contiguity applies only to visceral-autonomic-involuntary effectors whereas effect applies only to skeletal-central-voluntary effectors is intriguing but unconvincing. Skeletal responses can be conditioned and primarily visceral responses can be selectively learned. At least this is the way I interpret eyelid conditioning and the use of rewards and punishments in teaching control of bowel and bladder sphincters.

Moreover, the voluntary-involuntary breakdown does not seem to be particularly neuro-anatomical. It is well known that many visceral responses can become voluntary, and skeletal responses which are not ordinarily useful never come under voluntary control. Few people can wiggle their ears, cross their eyes at will, or move each toe independently, although all of these can become voluntary with practice. A reasonable alternative to the Mowrer hypothesis is that while skeletal responses are continually determining whether rewards are forthcoming, visceral systems rarely get exposed to selective learning. Instead, visceral systems are reared in the constant and predictable type of environment which promotes the evolution of innate reflexes and prevents selective learning from getting a chance to occur. However, selective learning apparently can take over in the autonomic when, for example, a conflict makes acquisition of a psychosomatic symptom rewarding.

These foregoing single-process arguments can of course be as well utilized by an effect theorist, and do not particularly support contiguity except in a roundabout way. I would now like to drop the role of devil's advocate and argue that there is so much room for theoretical expansion within the framework of *any* theory of selective learning that a contiguity approach is at least as promising as the effect one.

The room for expansion comes chiefly from the fact that nobody knows what "effect" is. It is an odd law that contains a term so indeterminate in advance. It is analogous to the law of sleep which states that "sleep is produced by soporific agents." As currently used, effect is defined as anything which strengthens instrumental responses. We can collect an empirical list of such events and get a workable statement of the law of effect as follows: "If a response occurs contiguously with a neutral stimulus pattern, it will become connected to that pattern if it is followed by one of the things in the list." However, this workable law gives only a circular description of the operations producing "effect." We are essentially defining effect as any event which makes the law of effect true. Descriptively this procedure may be practical but it leaves us up in the air about the outcome with an untried event. It also makes the law of effect exceptionless and untestable from a reductionist standpoint.

The reductive alternative to this handbook type of "law" is to seek a common factor in the empirical list of rewards. If we hit upon the right factor, we can predict the outcome when a new untried event is put in the reward position in a trial and error situation. The search for such a factor has been hampered because of one fairly common factor in the items of the empirical list collected so far. It is the property of being events which we feel are desirable from the standpoint of the organism. This leads to one of the most common and reprehensible versions of the law of effect—a version which merely says that responses with "adaptive" outcomes get fixated, and does not state the procedure for determining the adaptivity of a response. Such formulations probably constitute the most common way of conceptualizing the law of effect.

All such purposive conceptions of the law of effect are probably merely statements of the biological significance of learning. Selective learning is bound to be dominated by adaptive outcomes if the animal is left to his own devices in a maladaptive situation. He got the learning process through natural selection and it would be a surprise if it did not turn out to be a bodily process which helped him adapt to his environment. But survival-need reduction is

no more a cause of an individual instance of learning than is cooling of the body a cause of a person's perspiring when hot.

A more mechanistic, and therefore more acceptable, interpretation of effect is the drive reduction hypothesis of Mowrer (13), Miller and Dollard (11), Hull (8), and others in the S-R reinforcement group. There are numerous difficulties in Hull's use of "need reduction," but when he and his followers speak of "drive reduction"—or still more tangibly, of "stimulus-intensity reduction"—we have a principle which can predict that most learning will be adaptive but also can account for such examples as the maladaptive drug addict who acquires a habit which is adjustive but not adaptive.

But a good mechanist is also not going to ignore the Smith and Guthrie (21) hypothesis that effect is removal from the situation. If one is endowed with a variety of adaptive reflexes, what could be more adaptive, when next thrown into a previously experienced maladaptive situation, than to do whatever got you out of it the last time you were in it? The response that changes a maladaptive situation into an adaptive one is inevitably going to be a wise choice from an adaptive standpoint—and a last-response factor may well have been incorporated into the learning process by natural selection.

The Smith and Guthrie hypothesis—which they deduce from contiguity—is a hard one to test experimentally and it has survived without real disproof for about thirty years. It has the advantage over the drive reduction position in that it accepts contiguity as sufficient and therefore does not have the awkwardness of the drive reduction position in coping with the conditioning of such responses as the knee-jerk, vaso-dilation, salivation based on acid, and so forth. Moreover, the drive reduction position is faced with problems even in its home territory of accounting for instances of *selective learning* where the "reward" is not drive reducing in any obvious way.

Thus in a collaborative study with Dr. Thornton Roby (17) we found that hungry rats would learn responses for which reward was only a very sweet solution of saccharine and water. Hungry rats gorge themselves on such a non-nourishing solution and even after weeks of acquiring sophistication in this form of sham-feeding they readily learn to dash to the correct side of a T-maze to get a few minutes at the saccharine bottle.

In some other experiments in collaboration with Jepson Wulff and Robert Backer (18) we found that male rats reared from infancy in segregation from females readily learned an instrumental response that led to a chance to copulate with a female in heat even though copulation was always stopped short of ejaculation. That is, the "reward" for selective learning was an opportunity to initiate copulation but the female was always removed considerably before ejaculation. By some standards the "reward" would be called sexual frustration. The animals had never ejaculated before the experiment so acquired drive reduction was ruled out; and they did not ejaculate during the experiment so primary drive reduction was ruled out. Yet the response instrumental in producing the female in heat showed steady acquisition compared with controls receiving only a male companion in the goal box.

These findings seem contrary to the drive-reduction hypothesis of the nature of "effect," although they might be thought to follow the beneception mechanism of Troland (22). Perhaps the proper generalization is that the common factor is the execution of an innate and prepotent consummatory response, regardless of whether it reduces

the drive state. Whatever the correct inductive generalization, the results are more in line with a Guthrian contiguity analysis of selective learning than they are in line with a Hullian drive-reduction analysis. According to Guthrie the common factor in the empirical list of rewards is that they radically change the environment from just before to just after reward. The kinds of changes Guthrie (5) considers important indicate that it should be unimportant whether or not the animal's drive is reduced so long as the consummatory behavior remains prepotent and therefore able to take the animal "out of the situation."

I cite these experimental findings more to prove the fluid stage of our knowledge about reinforcement than to bolster the Guthrian contiguity analysis. My opinion is that Guthrie is still a long way from exhausting the implications of contiguity for instrumental learning. In the meantime my hunch is that we will eventually be forced to conclude that rewards are critical only as the means of motivating practice and performance and that the contiguity factor will turn out to be sufficient in describing the effects of practice *per se*. In terms of the main topic of our symposium my chief argument is that disproof of the universality of the law of effect does not force us into dualism as the only alternative—at least not until an all-out modern attempt has been made to see if association by contiguity is the common process both in selective learning and conditioning.

BIBLIOGRAPHY

1. BIRCH, H. G., AND BITTERMAN, M. E. Reinforcement and learning: the process of sensory integration. PSYCHOL. REV., 1949, 56, 292–308.
2. BROGDEN, W. J. Sensory pre-conditioning. *J. exp. Psychol.*, 1939, 25, 323–332.
3. ESTES, W. K. Toward a statistical theory of learning. PSYCHOL. REV., 1950, 57, 94–107.
4. GUTHRIE, E. R. *The psychology of learning.* New York: Harper, 1935.
5. ——. The effect of outcome on learning. PSYCHOL. REV., 1939, 46, 480–484.
6. ——. Association and the law of effect. PSYCHOL. REV., 1940, 47, 127–148.
7. HILGARD, E. R., AND MARQUIS, D. G. *Conditioning and learning.* New York: Appleton-Century, 1940.
8. HULL. C. L. *Principles of behavior.* New York: Appleton-Century, 1943.
9. JENKINS, W. O., AND STANLEY, J. C. Partial reinforcement: a review and critique. *Psychol. Bull.*, 1950, 47, 193–234.
10. MAIER, N. R. F., AND SCHNEIRLA, T. C. Mechanisms in conditioning. PSYCHOL. REV., 1942, 49, 117–134.
11. MILLER, N. E., AND DOLLARD, J. *Social learning and imitation.* New Haven: Yale Press, 1941.
12. MILLER, S., AND KONORSKI, J. Sur une forme particulière des réflexes conditionelles. *C. R. Soc. Biol. Paris*, 1928, 99, 1155–1157.
13. MOWRER, O. H. On the dual nature of learning—a re-interpretation of "conditioning" and "problem-solving." *Harvard educ. Rev.*, 1947, 17, 102–148.
14. ——, AND LAMOREAUX, R. R. Fear as an intervening variable in avoidance conditioning. *J. comp. Psychol.*, 1946, 39, 29–50.
15. ——. *Learning theory and personality dynamics.* New York: Ronald Press, 1950.
16. SHEFFIELD, F. D. Avoidance training and the contiguity principle. *J. comp. physiol. Psychol.*, 1948, 41, 165–177.
17. ——, AND ROBY, T. B. Reward value of a non-nutritive sweet taste. *J. comp. physiol. Psychol.*, 1950, 43, 471–481.
18. ——, WULFF, J. J., AND BACKER, R. Reward value of copulation without sex-drive reduction. *J. comp. physiol. Psychol.*, 1951, 44, 3–8.
19. SKINNER, B. F. *The behavior of organisms.* New York: Appleton-Century, 1938.
20. ——. Are theories of learning necessary? PSYCHOL. REV., 1950, 57, 193–216.
21. SMITH, S., AND GUTHRIE, E. R. *General psychology in terms of behavior.* New York: Appleton, 1921.
22. TROLAND, L. T. *The fundamentals of human motivation.* New York: Van Nostrand, 1928.

REFLECTIONS AND CONFESSIONS OF A REINFORCEMENT THEORIST [1]

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During the last two decades it has become customary for some psychologist, every so often, to take a pot shot at the reinforcement principle. These attacks have usually taken the form of an analysis of certain data followed by the *inescapable* conclusion that the reinforcement principle is untenable.

The reinforcement psychologists, known for their passionate devotion to their principles—especially their principle of reinforcement—have not responded to these onslaughts in anything resembling an affable and gracious manner. These assaults upon the authority of the reinforcement principle have instigated vigorous and aggressive counter-attacks. About a year following the appearance of the "uncalled for" criticism, a series of articles begin to appear. Some member of the reinforcement army reports new evidence which definitely refutes some non-reinforcement formulation. Another member reanalyzes the data initially offered in opposition to reinforcement theory and discovers to his surprise that the data, rather than being contradictory, really confirms reinforcement theory. While all of this is taking place another article—or perhaps a book—is published which modifies some aspect of S-R reinforcement theory in order to increase its explanatory powers.

Now this attack by non-reinforcement psychologists, followed by the vigorous defense of the reinforcement behavior theorists, has occurred time and time again. This routine, which at times has become exceedingly dull, is not going to be repeated here. Rather than become defensive about S-R reinforcement theory, permit me to become somewhat offensive about other formulations.

The APA meeting is certainly not an appropriate place for a sermon, but if I were to give a sermon, I would use as my text the following quotation from page 36 of Chapter V of the book *On Understanding Science* by President Conant of Harvard University:

"We can put it down as one of the principles learned from the history of science that a theory is only overthrown by a better theory, never merely by contradictory facts."

The question which I wish to raise is whether there is at the present time a more adequate theory of learning than the S-R reinforcement theory of Hull, Spence, Miller and their co-workers? Or to phrase the question somewhat differently: What have other theories to offer in competition to a theory that has made serious, and many successful, attempts at explaining the data in the fields of the acquisition and extinction of conditioned responses (12), discrimination learning (26), maze learning (8), human rote learning (13), transposition (27), response fixation (6), social learning (21), motor skills (1), dis-

[1] The polemical nature of this paper stems mainly from what the author considered to be the "social expectancies" of a paper delivered at an APA symposium on a very controversial topic. Rewriting the paper for publication would have required extensive revisions which would have been at odds with the agreement of the members of the symposium—to publish essentially what they said at the meeting. If the above paper had been "rewritten for publication" it would have said the same thing, except in a much more guarded and dispassionate manner.

tribution of practice (15), learnable drives (20), psychotherapy (5), etc.?

So many of the non-reinforcement psychologists have been so obsessed with the need to criticize the reinforcement position that they have expended most of their intellectual resources on such criticism while ignoring the far more important task of developing the explanatory capacities of their own formulations. If it has been the intent of these critics to weaken the reinforcement position, the fruits of their labor have betrayed their motivation. This constant barrage of criticism leveled at the reinforcement position has been one of its main sources of vitality and productivity.

But let us now return to those theories which are offered in competition to the S-R reinforcement formulation and let us remember "that a theory is only overthrown by a better theory, never merely by contradictory facts."

The first formulation I wish to evaluate, and frankly the one most vulnerable to critical analysis, is the formulation hinted at by Maier and Schneirla in their book (18) published in 1935, expanded in their article (19) published in 1942, and modified by Birch and Bitterman (2) in 1950. Is this a "better theory" than the S-R reinforcement formulation? Conant (3) suggests that we should evaluate an attempt at theory construction both in terms of its success or failure in stimulating experimentation and in terms of its success or failure in explaining the known facts. In terms of the former requirement the evidence—or rather the lack of evidence—is rather convincing. I am not aware of one single bit of research which was stimulated directly by the Maier-Schneirla-Birch-Bitterman two factor learning theory. By chance alone one would expect some research to be instigated by an idea which had its conception fifteen years ago and its birth eight years ago. The sterility of this particular two factor theory can certainly not be attributed to the lack of energy of its authors. Dr. Bitterman has presented an experimental paper at several of the meetings I have attended in the past few years. All of them were designed to test some implication of the S-R reinforcement theory. Why, may I ask, did he not attempt to test some implication of the sensory integration principle? I believe this question can be answered by an analysis of the Maier-Schneirla-Birch-Bitterman formulation.

Maier and Schneirla in 1942 insisted that there were two qualitatively different forms of learning. They wrote:

> ". . . in the *first stage* of the conditioning *procedure* a neutral stimulus develops excitatory value for a response it previously did not control. The essential condition for this change in the animal is contiguity between the experience of the neutral stimulus and the experience of the unconditioned stimulus. Then a *second* stage may develop. Once the neutral stimulus has become a conditioned stimulus, the response it elicits may be rewarded or punished" (19, p. 131).

That is, in the classical conditioning set-up, sheer contiguity of the conditioned and unconditioned stimuli is the necessary condition for establishing the initial associative connection, with reward playing no role. Following this initial stage, the second stage may develop. Once the response anticipates the unconditioned stimulus, selective learning takes place. Reward then functions and serves to strengthen the association and perhaps modify the response.

Dr. Underwood and I (14) examined the experimental data offered by Maier and Schneirla in support of their postulating an initial contiguity stage of learning. We concluded, like good reinforcement theorists, that the data offered by them could be integrated within

S-R reinforcement theory. I believe we gave valid reasons. We also went one step further. We suggested some experimental tests of their formulations. Perhaps this is not the proper thing to do. Birch and Bitterman (2) at least did not think so since they dismissed our proposal, as being "irrelevant" in spite of the fact that they were based upon Maier and Schneirla's *explicit statement* that there were two distinct stages of conditioning. Actually the reason these proposals were considered irrelevant was due to the fact that Birch and Bitterman (2) in replying to Underwood and my criticism of Maier and Schneirla's two stage theory of learning surreptitiously substitute the term "process" for "stage" and state that the two processes (contiguity and reinforcement) "are not situationally defined." They also state that "under certain conditions both (contiguity and reinforcement) must be assumed to occur."

What does this sort of discussion mean? It means that by not defining these "processes" situationally Birch and Bitterman have effectively protected their formulation against *any possible* empirical attack. It means that, by their failure to provide these experimental specifications, combined with their assumption that both processes of learning can occur simultaneously, they have created an illusory formulation that seemingly can incorporate all the discordant learning data. Actually, their formulation explains nothing.

This lack of contact between the concept of sensory integration and independent manipulatable variables is not surprising when we consider the type of logic underlying its proposal. After rejecting their personal interpretation of Hullian theory, Birch and Bitterman conclude that "there is no alternative but to postulate a process of sensory integration to account for stimulus substitution." This type of formulation may be aptly described as "negative theorizing," *i.e.*, the validity of one viewpoint is demonstrated by the fallaciousness of another viewpoint.

What surprises me most is Birch and Bitterman's dogmatic insistence that "there is no alternative but to postulate a process of sensory integration . . ." when they so willingly admit ". . . we know very little about the conditions under which sensory integration occurs."

It seems to me, and I am now speaking as a clinician, that the term "sensory integration" must have rich connotative meanings to Birch and Bitterman in spite of obvious lack of objective references. These connotative meanings as I have attempted to demonstrate have neither heuristic nor explanatory value. I fear Birch and Bitterman have committed an error which is common to many physiologically oriented psychologists. They have failed to appreciate the fact that physiological speculation is not equivalent to scientific explanation. Adequate explanatory systems, involving either physiological or behavioral mechanisms, must have deductive components (7).

Let us now turn our attention to the two other points of view represented in this symposium. Both possess a great deal more substance than shadow. It would be most beneficial to psychology if both theoretical formulations were more fully developed so that we could adequately estimate their true potentialities. To be quite frank, in spite of my own stated preferences, I feel that a disproportionate amount of research and theorizing has been done within the reinforcement framework. At our rather primitive state of development it would be unfortunate if we directed most of our energies in one direction. I might add, however, that we obviously cannot place the blame for the present "unequal distribution" upon the shoulders of the reinforcement psychologists.

My major objection to Mowrer's two factor theory (22) is not based upon any inherent aversion to two factors. To be quite truthful, I have no valid objection to even three basic factors—or four—or five—or even as many as Tolman would like to have. But what I do object to is that all the multiple process theorists have failed to come to grips with the positive construction aspects of their theorizing. Their formulations have essentially developed from negative notions, *i.e.*, a single factor contiguity or effect theory is not adequate. No amount of criticism of other formulations can substitute for the very difficult and laborious task of developing a logical, meaningful theory. The problem of sharply distinguishing between two major factors in learning is a difficult one. It must be done in such a way that it is possible to ascertain the influence—or lack of influence—of each factor at any one time. Both mechanisms must be adequately anchored to experimental variables. If both mechanisms are assumed to operate simultaneously—and this certainly can be a legitimate assumption—it becomes necessary to specify in what manner each mechanism contributes to the behavior pattern which is observed. If the above requirements are *not met,* multiple process formulations will be nothing more than a group of theoretical escape clauses which, in any situation, can be conveniently invoked.

Mowrer (22) believes that conditioned responses involving skeletal muscle activity require reinforcement but contiguity is a sufficient condition for emotional conditioning involving responses controlled by the autonomic nervous system. Although this division at first glance appears to be clear, confusion arises when an experimental situation involving both skeletal and autonomic responses is analyzed. I believe the experiment by Mowrer and Suter (with which I am familiar only through secondary sources) demonstrates this basic difficulty.

It may be recalled that these experimenters trained two groups of rats in a situation in which shock served as the unconditioned stimulus. For one group the conditioned stimulus ended with the onset of the shock while for the second group the conditioned stimulus was terminated at the end of the shock. Mowrer and Suter reasoned that according to reinforcement theory the second group should evidence greater fear. This would follow, according to them, because the conditioned stimulus persisted, for the second group, until reinforcement occurred. Since no difference between the groups was obtained, Mowrer and Suter concluded that contiguity alone could account for conditioned fear. As Dr. Sheffield has indicated, the experimental operations do not adequately reveal what responses are occurring and what responses are receiving reward. Dr. Miller, in an unpublished article (20), suggests another interpretation:

> "It is possible that the crucial thing that had to be reinforced by drive-reduction was the response of starting to be afraid at the onset of the conditioned stimulus and that this response is different from the one of maintaining an already elicited fear while the conditioned stimulus persists. If this should be true, the experiment would not be a crucial test because the interval between starting to be afraid at the onset of the conditioned stimulus and the drive-reduction produced by the turning off of the shock was the same for both groups."

Although the results of the Mowrer and Suter experiment have been presented as being embarrassing to a single factor reinforcement theory, I believe it is more of a challenge to Mowrer to sharpen the empirical operations dividing his proposed two factors.

And now for some brief comments about the single factor contiguity theory. I find it very difficult to disagree with most of the comments made by Dr. Sheffield. This is not meant as an attempt at reciprocating for the able defense of reinforcement theory offered by Dr. Sheffield at several points in his paper (24). The truth of the matter is that if I were to abandon the reinforcement position I believe I would adopt the single factor contiguity position. I, too, am very impressed with the similarities between the so-called different types of learning. In addition, my preference is for a single factor theory because of the complicated logical problem which develops when multiple processes are assumed.

Implicit in Sheffield's request that an "all out modern attempt" be made to develop a single factor contiguity theory, is the realization that the contiguity "theory" at present is not a theory, but really a hope for one. In spite of the ingenuity of Guthrie's writings (10) its empirical productivity has been practically nil. One may be tempted to explain this on the basis that the scenic northwest is not the proper environment for serious system building. I believe a careful reading of Guthrie will indicate that there may be other reasons, in addition to the geographical ones, which have permitted contiguity theory to remain sterile for so long. Guthrie writes:

"The principle of conditioning is not an explanation of any instance of learning. It is merely a blank form which explanations of instances of learning may take. It expresses only the conviction that we may profitably look for the signs of a movement along those stimuli which are now present and which were once before present with the movement." [2]

[2] The importance of this passage was pointed out to the author by Professor Sigmund Koch.

It seems to me that Guthrie adopted an extreme positivistic viewpoint which is basically antithetical to theory construction. By refusing to consider contiguity as a general explanatory mechanism the search for sets of relationships between the mechanism of contiguity and stimulus and response variables was effectively stymied. I hope that by a "modern attempt" Sheffield is referring to the deductive type of theory initially espoused by Tolman (28). I think if he does, the psychology of learning will be greatly benefited by his undertaking.

I do not wish to leave you with the impression that the general undeveloped nature of the formulations discussed is their only drawback. I feel that there is some empirical evidence which is somewhat embarrassing to them. How does Dr. Mowrer explain the failure of Hilgard, Dutton, and Helmick (11) to condition the autonomic pupillary response? How would Dr. Sheffield explain the results of Loucks (16) and Loucks and Gantt (17) who report that contiguity alone was not sufficient for conditioning to occur? Only when reinforcement, in terms of either food or shock reduction, was present were these experimenters able to obtain conditioning. Could Drs. Birch and Bitterman provide any reasonable explanation of the work of Grice (9) and Kimble (15) which fits so snugly into S-R theory? Or could they even give us an adequate explanation of why sensory preconditioning occurred in some situations and not in others?

In closing I would like to make some confessions. Firstly, I am *not* a reinforcement theorist because I am convinced that reinforcement is necessary for learning. I am a reinforcement theorist purely for pragmatic reasons. During the past twenty years, reinforcement theory has been the most fruitful theory both in terms of its ability to instigate experimental research and in

terms of its ability to integrate various phenomena within its theoretical structure. For these reasons alone I feel justified in conducting my experimental work within the framework of S-R reinforcement theory.

The above does not imply that I am completely satisfied with the status of reinforcement theory. To a large extent the use of the term theory is a compliment that it does not sufficiently deserve. In spite of its relative sophistication, S-R reinforcement theory is still basically a primitive formulation. It would be unwise for S-R psychologists to try to "sell it" for anything more than that. It would be also unwise for S-R reinforcement psychologists to develop a "party line mentality" which would in any way discourage experimental work within other theoretical systems. The history of science is filled with examples in which a useful concept served as a barrier to the acceptance of a better one.

I also feel that there has been a disproportionate concern with the concept of reinforcement and specific theories of reinforcement. This condition can be, however, largely attributed to the critics of reinforcement theory. Certainly the data in psychology are not going to collapse to the principle of reinforcement alone. Rather than worry whether reinforcement is basically "need reduction" or some other type of mechanism I believe it would be more strategic to accept a general reinforcement theory which does not specify the underlying mechanism. When our motivational theory becomes more adequate (and great strides have been made in recent years), then some of our present problems concerning reinforcement will be clarified.

I do not want my emphasis of Conant's statement that "a theory is only overthrown by a better theory, never merely by contradictory facts" to be interpreted as a tacit acceptance of the existence of experimental data which are contradictory to effect theory. It is interesting to note that the major findings which have been offered in opposition to reinforcement theory consist of simple demonstrations of certain phenomena. An example of this is the phenomenon of sensory pre-conditioning. We know very little about this phenomenon. How is it modified by temporal relationships? How is it modified by the intensity of the stimuli? What responses are occurring during the pairing of the two stimuli? When these questions are answered I would not be surprised to find sensory pre-conditioning data fitting neatly into reinforcement theory.

The same situation existed in regard to the problem of partial reinforcement. After this phenomenon was systematically investigated, the results appear to be more easily integrated within S-R than within cognitive theory. Rubin (23), in work done at New York University, demonstrated greater resistance to experimental extinction by a group of animals reinforced 100 per cent as compared to a 50 per cent reinforced group. In his study, distributed training was used and secondary reinforcement was controlled. These data, of course, are quite consistent with the recent results reported by Virginia Sheffield (25) and Denny (4).

In closing, perhaps it would be best to repeat my major point. In the present competition between various learning theories, S-R reinforcement theory is far ahead in the race. May I extend a challenge and invitation to other theories to catch up?

BIBLIOGRAPHY

1. AMMONS, R. B. Acquisition of motor skill: Quantitative analysis and theoretical formulation. PSYCHOL. REV., 1947, 54, 263–281.
2. BIRCH, H. G., AND BITTERMAN, M. E. Reinforcement and learning: The process

of sensory integration. PSYCHOL. REV., 1949, 56, 292–308.

3. CONANT, J. B. *On understanding science.* New Haven: Yale Univ. Press, 1947.
4. DENNY, M. R. The role of secondary reinforcement in a partial reinforcement learning situation. *J. exp. Psychol.*, 1946, 36, 373–389.
5. DOLLARD, J., AND MILLER, N. E. *Personality and psychotherapy.* New York: McGraw-Hill, 1950.
6. FARBER, I. E. Response fixation under anxiety and non-anxiety conditions. *J. exp. Psychol.*, 1948, 38, 111–131.
7. FEIGL, H. Rejoinder and second thoughts in symposium on operationism. PSYCHOL. REV., 1945, 52, 284–288.
8. GRICE, G. R. An experimental study of the gradient of reinforcement in maze learning. *J. exp. Psychol.*, 1942, 30, 475–489.
9. ——. The acquisition of a visual discrimination habit following response to a single stimulus. *J. exp. Psychol.*, 1948, 38, 633–642.
10. GUTHRIE, E. R. *The psychology of learning.* New York: Harper, 1935.
11. HILGARD, E. R., DUTTON, C. E., AND HELNICK, J. S. Attempted pupillary conditioning at four stimulus intervals. *J. exp. Psychol.*, 1949, 39, 683–689.
12. HULL, C. L. *Principles of behavior.* New York: D. Appleton-Century, 1943.
13. ——, HOVLAND, C. I., ROSS, R. T., HALL, M., PERKINS, D. T., AND FITCH, F. B. *Mathematico-deductive theory of rote learning.* New Haven: Yale University Press, 1940.
14. KENDLER, H. H., AND UNDERWOOD, B. J. The role of reward in conditioning theory. PSYCHOL. REV., 1948, 55, 209–215.
15. KIMBLE, G. A. An experimental test of a two-factor theory of inhibition. *J. exp. Psychol.*, 1949, 39, 15–23.
16. LOUCKS, R. B. The experimental delimitation of neural structures essential for learning: The attempt to condition striped muscle responses with faradization of the sigmoid gyri. *J. Psychol.*, 1935, 1, 5–44.
17. LOUCKS, R. B., AND GANTT, W. H. The conditioning of striped muscle responses based upon faradic stimulation of dorsal roots and dorsal columns of the spinal cord. *J. comp. Psychol.*, 1938, 25, 415–426.
18. MAIER, N. R. F., AND SCHNEIRLA, T. C. *Principles of animal psychology.* New York: McGraw-Hill, 1935.
19. ——. Mechanisms in conditioning. PSYCHOL. REV., 1942, 49, 117–134.
20. MILLER, N. E. Learnable drives and rewards. In S. Stevens (Ed.), *Handbook of experimental psychology.* New York: Wiley, 1951.
21. ——, AND DOLLARD, J. *Social learning and imitation.* New Haven: Yale Univ. Press, 1941.
22. MOWRER, O. H. On the dual nature of learning—a re-interpretation of "conditioning" and "problem-solving." *Harvard educ. Rev.*, 1947, 17, 102–148.
23. RUBIN, L. An experimental comparison of the effects of partial and continuous reinforcement under conditions of different degrees of both secondary reinforcement and distribution of practice. Unpublished Ph.D. thesis. New York University, 1951.
24. SHEFFIELD, F. D. The contiguity principle in learning theory. PSYCHOL. REV., 1951, 58, 362–367.
25. SHEFFIELD, VIRGINIA F. Extinction as a function of partial reinforcement and distribution of practice. *J. exp. Psychol.*, 1949, 39, 511–526.
26. SPENCE, K. W. The nature of discrimination learning in animals. PSYCHOL. REV., 1936, 43, 427–449.
27. ——. The differential response in animals to stimuli varying within a single dimension. PSYCHOL. REV., 1937, 44, 430–444.
28. TOLMAN, E. C. Operational behaviorism and current trends in psychology. Proc. 25th Anniv. Celebration Inaug. Grad. Stud., Los Angeles: Univ. of Southern California, 1936, pp. 89–103.

COMMENTS ON MULTIPLE-PROCESS CONCEPTIONS OF LEARNING

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When a leading exponent of one hypothesis becomes converted to another, the fact that he has been moved to confess the error of his former ways carries great conviction. The experimental evidence presented in Mowrer's paper (15), however, does not carry similar conviction. He cites Gantt's (3) observation that a conditioned change in the heartbeat regularly precedes any conditioned instrumental response. This certainly is congruent with the hypothesis that an autonomic response, such as fear, is learned first and serves as the drive to motivate the instrumental response. But it does not prove that the autonomic response is acquired by a different type of learning—conditioned by contiguity instead of being reinforced by drive-reduction. The other experimental evidence, the study by Mowrer and Suter (16, pp. 275–283), has already been shown by both Kendler (7) and Sheffield (17) to contain serious flaws which make it inconclusive. Moreover, Mowrer (15) himself admits that the results are not crucial. This leaves us with only a hypothetical experiment to test a deduction from the drive-reduction hypothesis and the assertion: ". . . it would be rather amazing if the deduction were confirmed."

In view of the extreme weakness of this experimental evidence, I venture to suggest that another factor may have played a major role. In order to stick to a consistent drive-reduction theory of reinforcement, one is forced to assume that the reinforcement for learning to fear a painful event is not the onset of the pain, but its reduction. This seems to be a direct contradiction of all that is reasonable. It is to Mowrer's great credit that he has tried to formulate a consistent theory and thus faced this problem squarely. I agree that this application of the principle of drive-reduction goes against common sense; I feel uncomfortable every time I am forced to make it (2, 12, 13). Once in a great while, however, when common sense is tested, it proves to be wrong: an utterly unreasonable assumption, like a ray of light being curved by gravity, or a light stone falling as fast as a heavy one, turns out to be correct. That is the reason for stubbornly insisting on conclusive experimental evidence.

Let us return to Mowrer's (15) hypothetical experiment. According to a strict drive-reduction theory of reinforcement, the end of the noxious stimulus must be a reinforcement. Therefore, if the signal occurs immediately before a noxious stimulus, more fear should be learned if the noxious stimulus is brief than if it is long. This follows because (a) the interval between the signal and the reinforcing termination of the noxious stimulus will be lengthened if that stimulus is prolonged, and (b) longer intervals are less favorable to reinforcement. Therefore, other things equal, a signal followed by a brief noxious stimulus should acquire the capacity to elicit stronger fear than one followed by a prolonged noxious stimulus.[1]

[1] Actually the results might be obscured by two complicating factors: (1) the intensity of the painful stimulation might be increased enough by temporal summation so that the greater amount of reduction when it ceases

This unreasonable deduction is particularly interesting because it happens to have been tested recently in an unpublished experiment by Sullivan (22). Although Mowrer had no way of knowing it, the experiment he described is real rather than hypothetical. Sullivan used a change in a pattern of lights as the CS, a 90 decibel, 1,000 cycle tone as the US, and recorded the galvanic response of human subjects. For Group 1 the change in the pattern of lights was followed by a brief piercing tone (.75″). For Group 2 it was followed by a longer piercing tone (4.0″). Both the frequency ($p = .05$) and the amplitude ($p = .06$) of conditioned responses to the light were greater for the group having a brief, loud tone as the US. Furthermore, although there was no reliable relationship between the amplitude of the UR and the frequency of the CR, there was a relationship within each group between the rate of decrement of the UR and the frequency of the CR ($p = .14$ for Group 1 and .03 for Group 2). Thus, amazingly enough, the unreasonable deductions from a strict stimulus-reduction theory of reinforcement seem to be confirmed. This experiment should be repeated, however, with a control for the possibility that the responses of Group 2 were depressed by a greater amount of negative adaptation which may have occurred during the presentations of the longer tone.

In another of the papers in this symposium, Birch and Bitterman (1) have argued that the facts of evolution, the diversity of animal species, mean that there must be different mechanisms of learning. This does not necessarily follow. Birch and Bitterman could just as well argue that the obvious differences between the bodies of Rita Hayworth and an artichoke mean that it is ridiculous to assume that the same fundamental mechanism of cell division by mitosis is involved.

I agree completely with Birch and Bitterman that great differences in the intellectual capacity of individuals and species pose an important problem which has been largely neglected. But these differences in intellectual capacity do not necessarily imply differences in the basic mechanism of learning. Insight and reasoning may turn out to be matters of performance, recombining and using habits rather than fixating them. In our present state of ignorance about the sources of intellectual differences, we cannot be certain whether or not they involve different types of learning.

To take an extremely simple example, the mechanism of a calculator with ten banks of keys and twenty of recording dials may be made out of exactly the same kinds of parts functioning according to exactly the same principles as that of one with only two banks of keys and dials. But one can use the 20-dial machine to get simultaneously all of the constants for computing a correlation (ΣX, ΣY, ΣX^2, ΣY^2, $\Sigma 2XY$), while one could not do this with the 2-dial one. Mere replication of identical functional units allows one to perform new operations; it increases the capacity of the machine.

Intellectual differences could be produced in still other ways without involving any new types of learning. The sense organs or central analyzers might be constructed so that a larger number of complex patterns or relationships would have distinctive cue value. The transient reverberating traces of cues

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more than compensates for the delay in the reduction, or (2) the effect of delaying the offset of the stimulus might be greatly diluted if a major part of the reinforcement should come from an immediate (perhaps transitory) reduction produced by adaptation (13, p. 35) immediately after the on-effect. Therefore, it would be desirable to try to secure electrical recordings, preferably centrally, of the effective strength of stimulation.

might last much longer. A greater number of distinctive cue-producing responses might be available. These would be differences in performance rather than learning.

Differences in the capacity for performance may influence the course of learning without necessarily involving different mechanisms of learning. Thus a seal is more likely to be rewarded for catching fish and a swallow for catching flies. But it seems likely that the greater capacity of the seal to learn to solve underwater problems and the swallow to solve aerial ones depends on the difference between flippers and wings rather than on basic differences in the mechanism of learning.

I emphasize this point because some of our controversies may arise from looseness in the definition of learning. Thus Birch and Bitterman (1) seem to use the terms "learning" and "behavioral modification" synonymously. This enables them to bring in the example of the paramecium rendered more flexible by immersion in a solvent—a phenomenon which presumably is no more an example of learning than is the tanning of the skin on exposure to sunlight.

To change the subject, the brief sketch by Birch and Bitterman (1) of a new theory of the elimination of blinds is good because it starts to go into some of the details. I certainly hope that they continue to develop it with considerably more rigor and detail. Then we will have some basis for comparing it with Hull's (5) theory.

Paradoxically, a prospectus often seems more plausible than a more fully developed theory. This is because the difficulties become apparent only after one attempts to go into rigorous detail. For example, Birch and Bitterman (1) suggest that the facts of secondary reinforcement can be derived from a principle of sensory integration, established directly by contiguity and not dependent on drive decrement. This sounds plausible. But, on second thought, I believe it would force one to predict good "latent learning" in experiments, such as the one by Kendler and Mencher (8), which have conspicuously failed to show such learning. One would also deduce that the procedure of pairing a neutral stimulus with the presentation of food would be just as effective in establishing secondary reinforcement when the animal was satiated as when it was hungry.

The topic of latent learning brings up the claim of Birch and Bitterman (1) that the reinforcement theorists have pointedly ignored this topic because it is embarrassing to their theory. But stimulus-response psychologists have not neglected "latent learning." Instead of motivating the conspiracy of silence which Birch and Bittermann allege, reinforcement theory has pointed the way toward a theoretical and experimental analysis of so-called latent learning.

Studies like those of Tolman and Honzik (24) in complex mazes have been criticized as involving a large number of complex motivational factors such as reward from return to the familiar home cage, loss of fear, a possible exercise or exploratory drive, preferences for long unobstructed runs, aversion to being blocked at the end of blind alleys, etc. Thus the results of these experiments show an obvious improvement *before* the experimental group is rewarded and in the control group.

Because of the difficulties in controlling these factors in complex mazes, experiments have been performed in very simple T-maze situations. The general conclusion from these studies is that the little "latent learning" which sometimes occurs is a great deal weaker than the learning when drive and reward are known to be present. In short, "latent learning" is so relatively weak that it is much more plausible to assume that it

is produced by uncontrolled sources of motivation and reward than to assume that it represents a fundamental mechanism of learning (10). Thus the fact that a small amount of so-called latent learning sometimes occurs is not embarrassing to the reinforcement theorists; the fact that it is so weak should be extremely embarrassing to those who claim that all learning involves cognition or sensory integration established without reinforcement.

While competing theories have done little more than point to the fact of "latent learning" (ignoring its uncertainty and weakness), an analysis in terms of reinforcement theory has pointed to some of the variables determining whether or not it will occur. Meyers (11) has demonstrated that the sight of food has secondary reinforcing value even for satiated animals. Spence and his colleagues (21) have shown how fractional anticipatory goal responses based on secondary reinforcement could mediate "latent learning." They have made the reasonable assumption that, when an animal is given preliminary training in a maze containing an irrelevant goal object (*i.e.*, food when he is thirsty but not hungry) and a relevant one, there is conflict between the weak anticipatory goal response aroused by the irrelevant object and the strong one aroused by the relevant object. On the basis of this assumption they have deduced that the anticipatory goal response to the irrelevant goal object (and hence "latent learning") should be interfered with (1) the closer the relevant and irrelevant goal objects are in time or space, and (2) the stronger the drive for the relevant goal object. They have secured experimental evidence tending to support these deductions.

Similarly, as has already been pointed out (7, 17), those who make sensory integration or other purely cognitive principles the major factors in learning should be far more embarrassed by the extreme weakness and uncertainty of sensory preconditioning than the reinforcement theorists are by the fact that a little of it sometimes occurs. What we need is a theoretical and experimental analysis which will allow us to predict when sensory preconditioning will occur and when it will not. It will be interesting to see whether this analysis is produced by the reinforcement theorists or by their critics.

In dealing with the topic of sensory preconditioning Birch and Bitterman (1) raise the old centralist-peripheralist controversy. Though most S-R psychologists have been peripheralists, there is no logical necessity for this. One can assume that there are central responses which obey exactly the same laws as peripheral ones and produce stimuli which function exactly like those of external origin (2, 12, 13). The admission of central processes, such as perceptual responses and images, does not necessarily imply a different mechanism of learning. Central responses might even require less drive and reinforcement (since they are less effortful) and still be following the same laws as peripheral ones.

The concept of a central response differs from current cognitive theories in that such theories leave the laws of cognition vague while implying that they are different from those that the S-R psychologists apply to peripheral responses. For example, Tolman (23, p. 368) says with characteristic candor: "Wants and cognitions as interfused activities in the concrete apprehended behavior-fields do, I believe, function according to neo-Gestalt laws (which laws have, of course, for the most part not yet been either precisely discovered or stated)."

Kendler (7) has already pointed out some of the logical difficulties inherent

in multiple-factor theories. One must be able to distinguish each of the processes from the others. Mowrer (15) has made a clear-cut distinction on anatomical grounds, with responses of the somatic nervous system reinforced by drive reduction and those of the autonomic conditioned by contiguity. But when he includes *cognition* and *meaning* among the processes conditioned by contiguity (15), one wonders if he will consistently relegate them to the autonomic nervous system.

As Sheffield (17) has pointed out, the problem of making any distinction is complicated by the fact that the supposedly different types of learning seem to follow similar laws. To add to the difficulty, the definition of sensory integration has an important ambiguity. Birch and Bitterman say: ". . . when two stimuli are presented contiguously, the first acquires some of the functional properties of the second." Which properties? Such a principle is impossible to disprove, and hence is meaningless. Negative results can always be dismissed on the grounds that the particular properties in question are not acquired.

Contrary to some claims, an empirical definition of reinforcement is not completely *ad hoc*. The acquisition of a given habit is used to determine the fact that a certain event is reinforcing. This much is *ad hoc*. Then a prediction is made that the same event can be used to strengthen other S-R connections. This prediction could be either right or wrong; it obviously is not *ad hoc*. Definitions of this type have been elaborated elsewhere (2, 9, 13).

Sheffield (17) is correct in pointing out that such definitions result in a long and unwieldy list and in emphasizing the desirability of trying to find the common elements in the events included in the list. He calls attention to Guthrie's (4) suggestion that reinforcements are events that radically change the stimulus situation. I believe that this specification is too broad. Not all radical changes are reinforcing. For example, while the change from shock-on-the-grid to no-shock-on-the-grid will reinforce preceding responses, I do not believe the opposite change, from no shock to shock, will reinforce the responses that precede it. But turning on the shock takes the rat out of the situation of lounging at ease on the grid. All of the proprioceptive and emotional cues from sprawling out relaxed are removed when the shock is turned on and the animal starts jumping around. I would think that Guthrie would have to predict that turning on the shock whenever the animal scratched his ear would teach him this response, since it would always be the last response made to the situation of relaxed sprawling. Guthrie might argue that the jumping responses elicited by the shock are incompatible with scratching the ear. But the responses of ducking down to the food dish and eating are equally incompatible with pressing a bar above the food dish—indeed they sometimes interfere as anticipatory errors. Furthermore, ducking down to the food dish occurs as the last response before the situation is changed by eating while jumping occurs *after* the situation is changed by electric shock. Nevertheless the rat learns to press the bar if the changes produced by the delivery of food are contingent on that act. Then why should the animal not learn to scratch his ear if the changes produced by electric shock are made contingent on that act? The fact is that the direction and the magnitude of the change seem to be important.

Perhaps Guthrie's (4) theory can deal with the problem raised by the foregoing example, but the logic has not yet been worked out in enough detail to be convincing. It is conceivable that this

problem might be dealt with by refining Guthrie's theory to state that the removal of strong stimuli (involving the discharge of large numbers of neural units per second) produces a great effect while the removal of weak stimuli produces little effect. This would reduce the difference between Guthrie's theory and the version of drive-reduction that defines a drive as a strong stimulus (13).

It seems to me that in all of the cases (turning off strong shocks, bright lights, piercing sounds, etc.) that are most clear cut, reinforcement involves the reduction in an intense stimulus, or drive. The exceptions—saccharin (18), sexual excitement (19), etc.—seem to me to be complex cases in which it is more difficult to determine with certainty whether or not drive-reduction is involved.

Even in these difficult cases I believe there is a certain practical advantage in stubbornly sticking to drive-reduction, if only to infuriate someone into designing a superior theory backed by convincing experiments. For example, those who find it easy to postulate multiple processes or do not believe in theory are not likely to be spurred into action by the Sheffield and Roby (18) experiment showing that drinking a saccharin solution is reinforcing. But a strict drive-reduction theory demands that the saccharin must produce its reinforcing effect by temporarily reducing hunger (or some other drive), even though it does not seem to serve any need. Following this line of reasoning Mr. Kohn and I (14) have measured hunger by Skinner's (20) technique of bar-pressing reinforced at fixed temporal intervals, and have secured preliminary evidence that drinking a saccharin solution reduces the rate of bar-pressing more than drinking the same volume of water. In short, the sweet taste seems to produce some immediate drive-reduction. We are now in the process of checking up on this more carefully by comparing the effects of saccharin ingested by mouth with those of saccharin injected through a fistula directly into the stomach.

Having emphasized the drive-reduction hypothesis, I want to remind you that reinforcement theories of learning are not necessarily limited to that hypothesis. One can assume that there are other sources of reinforcement or be content with an empirical definition (2, p. 42).

I have stressed the value of trying to stick to the most parsimonious theory that will account for the facts. In conclusion I would like to point out that natural selection often operates on the "spare tire" principle rather than on parsimony. This may result in alternative mechanisms that are basically different. If this is true, the most parsimonious theory that will account for all of the facts may be quite complex. There is no objection to outlining programs for the development of multiple-process or other theories of learning. But such outlines are no substitute for the hard work of constructing a rigorous theory. Until rival theories of learning are developed to at least the primitive level of current reinforcement theory, they are not likely to displace it.

BIBLIOGRAPHY

1. BIRCH, H. G., AND BITTERMAN, M. E. Sensory integration and cognitive theory. PSYCHOL. REV., 1951, 58, 355–361.
2. DOLLARD, J., AND MILLER, N. E. *Personality and psychotherapy: an analysis in terms of learning, thinking and culture.* New York: McGraw-Hill, 1950.
3. GANTT, W. H. Psychosexuality in animals. In P. H. Hoch and J. Zubin (Eds.), *Psychosexual development in health and disease.* New York: Grune & Stratton, 1949, pp. 49–50.
4. GUTHRIE, E. R. *Psychology of learning.* New York: Harper, 1935.
5. HULL, C. L. The concept of the habit-family hierarchy and maze learning. PSYCHOL. REV., 1934, 41, 33–54, 134–152.

6. ——. *Principles of behavior.* New York: Appleton-Century-Crofts, 1943.
7. KENDLER, H. H. Reflections and confessions of a reinforcement theorist. PSYCHOL. REV., 1951, **58**, 368–374.
8. ——, AND MENCHER, H. C. The ability of rats to learn the location of food when motivated by thirst—an experimental reply to Leeper. *J. exp. Psychol.*, 1948, **38**, 82–88.
9. MEEHL, P. E. On the circularity of the law of effect. *Psychol. Bull.*, 1950, **47**, 52–75.
10. ——, AND MACCORQUODALE, K. A further study of latent learning in the T-maze. *J. comp. Psychol.*, 1948, **41**, 372–395.
11. MEYERS, J. A. An experimental study of the reinforcing value of food for non-hungry rats. Unpublished Master's thesis, Univ. Iowa, 1949.
12. MILLER, N. E. Learnable drives and rewards. In S. S. Stevens (Ed.), *Handbook of experimental psychology.* New York: John Wiley, 1951.
13. ——, AND DOLLARD, J. *Social learning and imitation.* New Haven: Yale Univ. Press, 1941.
14. ——, AND KOHN, M. The effect of saccharin on strength of hunger. Unpublished manuscript, Yale Univ., 1951.
15. MOWRER, O. H. Two factor learning theory: summary and comment. PSYCHOL. REV., 1951, **58**, 350–354.
16. ——. *Learning theory and personality dynamics.* New York: Ronald Press, 1950.
17. SHEFFIELD, F. D. The contiguity principle in learning theory. PSYCHOL. REV., 1951, **58**, 362–367.
18. ——, AND ROBY, T. B. Reward value of a non-nutritive sweet taste. *J. comp. Psychol.*, 1950, **43**, 471–481.
19. ——, WULFF, J. J., AND BACKER, R. Reward value of copulation without sex-drive reduction. *J. comp. Psychol.*, 1951, **44**, 3–8.
20. SKINNER, B. F. *The behavior of organisms.* New York: Appleton-Century-Crofts, 1938.
21. SPENCE, K. W., BERGMANN, C., AND LIPPITT, R. A study of simple learning under irrelevant motivational-reward conditions. *J. exp. Psychol.*, 1950, **40**, 539–541.
22. SULLIVAN, J. J. Some factors affecting the conditioning of the galvanic skin response. Unpublished Ph.D. dissertation, State Univ. of Iowa, 1950.
23. TOLMAN, E. C. The nature and functioning of wants. PSYCHOL. REV., 1949, **56**, 357–368.
24. ——, AND HONZIK, C. H. Introduction and removal of reward and maze performance in rats. *Univ. Calif. Publ. Psychol.*, 1930, **4**, 257–275.

CAN REINFORCEMENT THEORY ACCOUNT FOR AVOIDANCE?

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How much of the S-R reinforcement hypothesis can be preserved in the face of evidence from (1) the avoidance learning experiments, (2) the sensory pre-conditioning experiments, and (3) the latent-learning experiments? This is one way of stating the issue discussed in this symposium.

The fact that the S-R reinforcement hypothesis has been precisely employed in many areas of behavior leads some psychologists (Kendler and Miller in this case) to consider the present question as unimportant. This attitude is expressed in the emphasis laid upon the small, ***although significant***, effects found in the sensory pre-conditioning and latent-learning experiments. Does the smallness of these effects justify us in the conclusion that the question is trivial?

A young man went to the father of his prospective bride with a question which he felt he must have answered. "Is Josie a good girl?" he asked. The girl's father replied by listing all her good qualities: her marks in school were good, she was a good worker, she was popular, and so on. But the young man did not seem satisfied by these replies and repeated, "Yes. But is Josie a *good* girl?" "Well," replied Josie's father, "there is one thing. Josie is just the tiniest bit pregnant."

If *any* learning, no matter how tiny, occurs in the sensory pre-conditioning and latent learning experiments, then one basic assumption of the S-R reinforcement hypothesis is false. It is ***not*** true that all learning consists in the strengthening of specific stimulus-response connections by reinforcement. The fact that Josie has many other good qualities does not make the present question a trivial one.

Since most of the talk in this symposium concerns the implications of avoidance learning for the S-R reinforcement hypothesis, I will restrict my discussion to this problem. The basic problem, as I see it, is this. What happens to the reaction-potential of the avoidance response when the response antedates the shock and no shock occurs?

Let us consider the experimental facts first, and then turn to the implications of each of the hypotheses presented in the symposium. There are many experimental situations in which the latency of the response increases after the failure to get shock, and as a consequence performance becomes unstable. There is, however, at least one experiment in which this does not occur. In Solomon's (3) experiment, there was, with normal dogs, no increase in latency, during 600 shockless trials. It is not true then that the ${}_sE_r$ of the avoidance response is *always* weakened by a shock-less trial.

The hypothesis of Kendler and Underwood (2) states that reinforcement is provided by termination of shock. Without such reinforcement reactive-inhibition accumulates and reaction potential decreases. Sheffield's contiguity hypothesis also depends upon reinforcement by shock. Reinforcement according to this hypothesis consists in the occurrence of an unconditioned stimulus for the learned response. Sheffield presents evidence that in ***his*** experiment, without such reinforcement, the

habit weakens. Thus neither of these hypotheses can account for the Solomon data without modification.

The sensory-integration hypothesis does not make any specific assertions about how un-learning occurs. If, however, it is asserted that un-learning results from S_1 occurring without S_2, then this hypothesis, too, would be unable to account for the Solomon data.

Finally let us consider what happens to the reaction-potential, according to Mowrer's and Miller's hypotheses, on each shock-less trial? Two effects occur: (1) the ${}_sH_r$ for the avoidance response is strengthened by the reduction in the learned fear drive, and (2) the ${}_sE_r$ for the learned fear drive is weakened because it is not followed by shock. How will these two effects then affect the ${}_sE_r$ for the avoidance response on the next trial? Both Miller and Mowrer follow Hull in regarding ${}_sE_r$ as a multiplicative function of ${}_sH_r$ and D. Thus the increase in ${}_sH_r$ for the avoidance response tends to increase the ${}_sE_r$ for that response. But what effect does the decrease in the ${}_sE_r$ for the learned fear drive have? First it will decrease D (*i.e.*, the magnitude, and probability of the fear response decrease, and its latency increases). As a consequence of the decrease in D, the ${}_sE_r$ for the avoidance response will be decreased. Thus the two effects of a shock-less trial are opposed. Can the Solomon data be accounted for by the further assumption that these opposed effects are equal and thus cancel each other, maintaining the ${}_sE_r$ for the avoidance response at a fixed level? I do not think that this assumption will solve the problem, if there is, as Hull says, a maximum value of ${}_sH_r$ for any habit. Once this maximum is reached, continuous decreases in D will be reflected in continuous decreases of the ${}_sE_r$ for the avoidance response. The shape of the acquisition curve during the first few trials in avoidance learning makes it scarcely plausible to assume that the maximum for ${}_sH_r$ is not reached soon in Solomon's experiment.

Since in all four of the hypotheses we have considered there is the basic assumption that the maintenance of the avoidance habit depends either directly or indirectly upon the contiguity of the conditioned stimulus and the shock, the Solomon data contradict all these hypotheses. The questions of (1) single or dual processes, (2) stimulus-response or sensory integration, or (3) contiguity or need-reduction, are all secondary questions. The fundamental question is whether any simple association hypothesis which is designed to account for *both* learning and extinction in other kinds of learning situations is also capable of accounting for the Solomon data.

In defense of the Miller-Mowrer hypothesis one might further hypothesize that learned fear responses have different extinction curves from those which characterize all other kinds of responses. Miller, in his discussion of the present problem, has suggested that such fear responses extinguish very slowly. But if one is to support this claim with experimental evidence, it is essential that the experimental criteria for the occurrence of the fear response be distinct from the criteria for the occurrence of the instrumental avoidance response. To point to the failure of Solomon's dogs to extinguish in 650 trials as evidence that fear responses extinguish more slowly than other kinds of responses, is to beg the question completely! For it is just as reasonable to conclude from this same evidence that instrumental avoidance responses, once acquired, do not depend for their maintenance upon continued reinforcement. This second interpretation is, as we shall see, the one proposed by the expectancy hypothesis. Finally, only if the advocates of the Miller-Mowrer hy-

pothesis can provide *independent* evidence concerning the extinction of fear responses, will their interpretation be preferable to the expectancy hypothesis.

Mowrer's suggestion that the fear response is a complex of visceral responses controlled by the sympathetic nervous system is a step in this direction. Could it be demonstrated (1) that such visceral responses are necessary for the acquisition and maintenance of the avoidance response, and (2) that such visceral responses are excessively difficult to extinguish, then the Miller-Mowrer hypothesis would be a satisfactory explanation of avoidance learning. But Solomon's evidence drawn from sympathectomized dogs shows that the first of these two conditions is not satisfied. Concerning the second condition there is no evidence at present that the extinction rates of responses controlled by the sympathetic nervous system are at all different from those of responses controlled by the somatic nervous system. In the light of these considerations, then, it seems that avoidance learning still poses one of the basic problems for reinforcement theories.

The hypotheses which we have so far considered assume that the habit learned in avoidance training is weakened on each occasion when the signal occurs without the shock. The Solomon data suggest that this assumption may be false. What about the expectancy hypothesis? Does it also make the same assumption?

One postulate of the expectancy hypothesis is that when a signal is not followed by what it signifies, the expectation aroused by the sign is weakened so that on the next occasion of the signal the expectation will be weaker, have a longer latency, and lower probability of occurrence. Thus the main problem is to specify what, according to the expectancy hypothesis, is signified by the signal. Hilgard and Marquis (1) in presenting the expectancy hypothesis appear to regard the shock as the signification of the signal. Given this interpretation, it is clear that the expectancy hypothesis must assert that when the signal occurs without the shock the expectation weakens, and this hypothesis is in no better position to interpret Solomon's results than the hypotheses we have already considered.

But the Hilgard and Marquis exposition of this hypothesis omits a characteristic of the signification which is essential. The signal signifies something with such and such properties *in* a particular spatio-temporal location. Without this double aspect of the signification there seems to be no reason for calling the dispositions aroused by the signal, "expectations." Thus should the signal merely arouse a preparatory disposition for, say caviar, in the sense that the animal is now eager to eat caviar, wherever he finds it, then there is no rationale for saying that this disposition is true or false. This disposition is simply an appetite for caviar. But when this appetitional disposition is combined with a spatial disposition so that when the signal is given the animal is prepared for caviar in a specific place and is not prepared for it in other places, then we may say that this compound disposition is true if caviar is in that place, and false if it is not. Such compound dispositions I call "expectations." They are strengthened by finding the signified object in the signified location. They are weakened by not finding that object in that location. Thus in a simple T-maze problem the expectation of food at L_A is strengthened by finding food at L_A and weakened by not finding food at L_A. It is unaffected, however, by either (1) finding food at L_B, or (2) not finding food at L_B, where L_B is any place which is discriminable from L_A. In the two latter cases only the strength of the ex-

pectation of food at L_B is changed. Consideration of these statements makes it clear that the behavior at the choice point cannot be taken as unequivocal evidence for the assertion that the animal expects food at location A, since the behavior at the choice point is determined by a variety of factors including the relative strengths of expectations of food at L_A and L_B. Instead the behavioral test for such expectations must be based, as Tolman has pointed out, upon what the animal does at L_A and L_B. In discussing M. H. Elliott's experiments on the effect of changes in the goal, Tolman says: "On the trials immediately after the change, Elliott observed a disrupted, searching sort of behavior. . . . The point we would make is that such 'searching,' *i.e.*, disruption, is to be taken as the empirical evidence for, and definition of, an imminent expectation of the previously obtained bran mash" (4, p. 74). In experiments at the University of California precise behavioral tests for such expectations may enable us to measure expectation-strength independently of the animal's behavior at the choice point. When this has been accomplished, we will be in a position to discover how and when such expectations affect choice point behavior.

Let us return now to a consideration of the application of the expectancy hypothesis to avoidance learning. What happens to the expectation when the animal responds to the signal and so succeeds in avoiding the shock? According to the present statement of the expectancy hypothesis the signal signifies shock at a specific place L_A. The animal is located at L_A at the time the signal is presented. As a consequence of the expectation aroused by the signal the animal responds by leaving L_A and moving to a different place L_B. The animal receives no shock at L_B. What effect should this consequence have upon the animal's expectation of shock at L_A? None! The only way of weakening the expectation of shock at L_A is to have the animal remain at L_A and fail to get a shock. As long as the animal moves from the region of the expected shock, the expectation must remain at its original strength. This hypothesis, then, will account for the fact, if it is a fact, that sometimes the response in avoidance learning never extinguishes. How can this hypothesis account for the fact that such responses often do extinguish?

It will be remembered from the preceding discussion that according to the present view the behavior at a choice point cannot be taken as unequivocal evidence for the assertion that an animal has a particular expectation. Many factors determine the choice point response and the expectation is only one of them. Now the same considerations apply to the avoidance learning problem. Thus although the animal may have the expectation at full strength, he may fail to make the avoidance response. Either fatigue or distraction or both might on one occasion inhibit the response. In such a case if no shock is given at L_A, the expectation will then be weakened. Then on the next occasion when the signal is given, a weaker expectation will be aroused and the probability of an avoidance response will be less. From then on, as each failure to respond is followed by no shock at L_A, the expectation is continuously weakened until it disappears entirely and as a consequence the response is extinguished.

Because there has been relatively little research upon the extinction of avoidance learning, there is at present no strong evidence for the expectancy hypothesis. On the contrary, there is some evidence in the Solomon experiment which appears to contradict the present hypothesis. I refer to the fact that Solomon was unable to produce

extinction of the avoidance response when he locked the normal dogs in the chamber and the signal was *not* followed by shock. If this procedure produced on each occasion a decrement in the expectation of the shock at L_A, as it should if the animal was expecting shock in that specific place, then according to the present hypothesis, the avoidance response should gradually extinguish. The basic question, then, is whether or not this procedure in Solomon's apparatus does weaken the expectation. We have at present no *independent* evidence that it did. Are there any reasons for supposing that it may not have had this effect? I believe there are.

It should be realized that Solomon's dogs in the original training received shocks in compartment A and compartment B equally often after the *same* signal. Because of this, and because both compartments were similar in character, it seems reasonable to suppose that the expectations aroused by the signal were ambiguous. Thus failure to receive shock in compartment A after the signal might not affect the strength of such an ambiguous expectation. What is needed to settle this question is evidence of the effects of this training upon the animal's expectations, evidence which is *independent* of the avoidance response.

It was emphasized earlier that if the Miller-Mowrer hypothesis were to become a satisfactory explanation of avoidance learning, independent evidence of the behavior of learned fear responses must be obtained. A similar problem faces the expectancy hypothesis. What is needed, here, before one can regard it as a satisfactory explanation, is independent evidence of the growth and decline of expectations. Because the avoidance learning problem forces us in this way to clarify our notions I regard it as one of the most fruitful areas for research in learning theory.

BIBLIOGRAPHY

1. HILGARD, E. R., AND MARQUIS, D. G. *Conditioning and learning.* New York: Appleton-Century, 1940.
2. KENDLER, H. H., AND UNDERWOOD, B. J. The role of reward in conditioning theory. PSYCHOL. REV., 1948, 55, 209–215.
3. SOLOMON, R. C., AND WYNNE, L. C. Avoidance conditioning in normal dogs and in dogs deprived of normal autonomic functioning. *Amer. Psychol.*, 1950, 5, 264.
4. TOLMAN, E. C. *Purposive behavior in animals and men.* New York: Appleton-Century, 1932.

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